Vol. III Part 4 November 1951

THE QUARTERLY JOURNAL OF EXPERIMENTAL PSYCHOLOGY

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CONTENTS

R. W. RUSSELL and R. G. F. PRETTY. A Study of Position Habits induced by Reward and "Frustration" 151

J. LANGDON. The Perception of a Changing Shape 157

H. KAY. Learning of a Serial Task by Different Age Groups - - - - - - - - - - 166

J. W. WHITFIELD. An Experiment in Problem Solving - - - - - - - - - - 184

Book Review - - - - - - - - - 198

PUBLISHED FOR
THE EXPERIMENTAL PSYCHOLOGY GROUP
BY
W. HEFFER & SONS LTD. CAMBRIDGE

AMERICAN PSYCHOLOGICAL ASSOCIATION

1515 Massachusetts Avenue, N.W., Washington 5, D.C.

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THE QUARTERLY JOURNAL OF
EXPERIMENTAL PSYCHOLOGY

Vol. III 1951 Part 4

A STUDY OF POSITION HABITS INDUCED BY REWARD AND "FRUSTRATION"*

BY

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Predictions from Maier's theory of "frustration"-instigated behaviour have been tested in an experimental situation differing significantly from that in which the theory was propounded yet containing the central element of "frustration"—the insoluble problem. A water discrimination unit was employed in which the performance of rats would be observed during attacks on insoluble problems, position problems or symbol problems.

Two groups, each containing ten Wistar albino rats, served as subjects. The research design consisted of the following phases: preliminary training, development of position responses, exposure to a symbol-reward problem with 50 per cent. punishment and exposure to a symbol-reward problem with 100 per cent. punishment. The design differed for the two groups only at the phase in which the position responses were established. During this phase one group was exposed to a position-reward problem and the other to an insoluble problem.

Position responses were established as frequently under position-"frustration" (position stereotypes) as under position-reward (position habits) conditions. Position stereotypes were more rigid—more resistant to extinction—than position habits under conditions of 50 per cent. punishment. Position stereotypes were as readily extinguished under 100 per cent. punishment as were position habits under 50 per cent. punishment.

The first two observations conform to predictions made from Maier's theory. The third does not. That is to say, not all situations containing the basic elements of "frustration" give rise to stereotyped behaviour patterns which are as rigid or "fixated" as Maier's theory would predict. It is a reasonable hypothesis that the characteristics of stereotyped responses established in certain "frustration" situations may be described adequately in terms of conventional learning principles without the necessity of resorting to a distinction between "goal-motivated" and "frustration-instigated" behaviour.

I

Introduction

The extensive work of Maier and his co-workers, summarized in his recent text on "Frustration: The Study of Behaviour Without a Goal" (1949), has important implications concerning the aetiology and nature of "abnormal" behaviour patterns. They have found that stereotyped behaviour patterns develop when subjects (rats) are forced to respond in an insoluble problem ("frustration") situation. Objectively observed, these behaviour patterns have much in common with position habits established under conditions of reward reinforcement. However, such stereotyped responses may differ very significantly from position habits in terms of their persistence when a soluble (symbol reward) problem is introduced after the stereotyped

* The Authors wish to acknowledge the assistance of Mr. A. R. Jonckheere and Mr. J. G. L. Williams during the collection of the data reported here and during the composition of the report.

† Fulbright Advanced Research Scholar and Director, Animal Psychology Laboratory.

response has been established. Stereotyped responses may show much greater resistance to extinction than do position habits. As a result of these observations, Maier proposes that "frustration-instigated" behaviour differs qualitatively from "goal-motivated" behaviour and, therefore, is describable in terms of different principles.

The present paper reports the first in a series of experiments designed to study the implications of Maier's theory. The majority of experimental data upon which this theory is based have been obtained by controlled observations of the rat's behaviour in the Lashley Jumping Apparatus. Systematic variations of environmental conditions have been introduced within the limits of this experimental situation. However, it seems logical that, if Maier's conclusions have the broad significance he imparts to them, similar results would be predicted in other experimental situations containing the elements of "frustration" as Maier has defined them. The present research has employed an experimental situation which differs from Maier's in the kind of motivation used, the type of response studied and the nature of the reinforcement employed, while retaining the "frustration" of the insoluble-problem situation. The results support predictions from Maier's theory regarding the development of stereotyped position responses, but show certain divergences from predictions concerning the rigidity of the stereotyped response once established.

II

Research Design and Procedure

A. Research Design

The research design employed in the present study is presented in Table I. Subjects were selected at random from the highly inbred laboratory population and throughout the research were treated identically except for the differences required by the research design.

TABLE I

Research Design

Group	*N*	*Phase* 1	2	3	4
Position-"frustration"	10	Preliminary training	Insoluble problem	Symbol problem: 50% punishment	Symbol problem: 100% punishment
Position-reward ..	10	Preliminary training	Position problem	Symbol problem: 50% punishment	Symbol problem: 100% punishment

This design allowed the testing of three predictions derived from Maier's theory. First, position responses, overtly alike in character, would develop at phase 2 in both experimental groups. Second, at phase 3 the position responses would undergo rapid extinction and symbol responses would be readily established in subjects of the position-reward group, but not in subjects of the position-"frustration" group. And, third, a shift to 100 per cent. punishment in phase 4 would not alter the position responses of the position-"frustration" animals.

B. Apparatus

The apparatus employed in presenting the soluble and insoluble problems was the water discrimination unit described by Russell, Jonckheere and Williams (1950). The animals were required to swim the length of a straightway in order to reach two doorways through which they could escape from the water. Each doorway contained a panel on which was painted a symbol. Below this panel was a door hinged in such a fashion that it could easily be pushed open by the animal. The symbols—black circle on white background or white circle on black—could be moved from one doorway to the other, and each door could be locked independently of the symbol above it. This made it possible to present to the animal any one of the problem situations required by the research design. A position problem could be presented by always locking the door on one side regardless of the symbol above it; a symbol problem, by always locking the door beneath the same symbol; and an insoluble problem, by shifting the symbols and locking the doors in independent random sequences. Motivation was controlled by maintaining the temperature of the water at 18° ± 1° centigrade. A response to the unlocked door was reinforced by immediate escape from the water, while a response to the locked door was punished by delaying escape until the animal swam around a partition between the doorways and pushed through the unlocked door. The difference between reward and punishment, therefore, was defined in terms of the duration of time in the water.

C. Subjects

Twenty male albino rats of the Wistar strain were used as subjects. One position-reward animal was rejected after phase 2 because of illness. All animals were approximately 230 days of age at the beginning of the experiment. During the hours between experimental trials they were kept in normal living cages in the animal room and maintained *ad libitum* on MRC diet 41A plus water.

D. Procedure

The procedures followed in each of the four phases of the research are described below.

1. *Phase 1: Preliminary Training.* Preliminary training was carried out in three stages. No symbols were presented during this period. All animals were given six trials per day for six days. Both doors were left open on the first three days, the animal being required to swim the length of the straightway and climb through a doorway to reach the escape platform. On the fourth and fifth days one of the doors was propped half open and released when the animals started to climb through; the other door was locked. Both doors were closed during the sixth day and the animals required to push the unlocked door open in order to escape. Preliminary studies had shown that such a training period was sufficient to orient the animal to the discrimination end of the water unit and to teach it the procedure for opening the doors.

2. *Phase 2: Position problem.* Twenty-four hours after the position-reward animals had completed their preliminary training they were placed in the water unit for their first learning trial. Ten trials were given per day until the animals had reached the criterion of 155 responses to the same position in the last 160 trials. This criterion was selected to correspond to the criterion employed by Maier and his co-workers in their research on "frustration." The unlocked door was always the one chosen by an animal on its first trial, thus corresponding to any position preference the animal may have had. The symbols were shifted at random but the locked door was always in the same position.

3. *Phase 2: Insoluble problem.* This same general procedure was followed with the position-"frustration" group except that the symbols were shifted and the doors locked in independent random sequences. Under these conditions the majority of animals developed position stereotypes to the criterion before 400 trials had been given. Those who did not reach this criterion were advanced to phase 3 of the research design at the end of the four-hundredth trial.

4. *Phase 3: Symbol problem, 50 per cent. punishment.* After completing phase 2 all animals were presented with a symbol problem in which the door below the black circle on the white ground was always unlocked regardless of the position in which it appeared. The symbols were shifted in a random sequence. Thus an animal who persisted in its position response was punished on 50 per cent. of its daily trials. Under these conditions all the position-reward animals and one of the position-"frustration" group reached the criterion of seven successive correct responses in one day's "runs" within 300 trials.

5. *Phase 4: Symbol problem, 100 per cent. punishment.* Those animals in the position-"frustration" group who did not solve the symbol problem with 50 per cent. punishment within 300 trials were advanced to phase 4 of the research design. In this phase an animal who continued to make position responses was punished on 100 per cent. of its daily trials. When the original position stereotype was broken the animal was rewarded for five consecutive responses to the opposite door. From then on any position response was punished on 100 per cent. of its trials since the objective of this phase of the research was to determine whether or not position stereotypes established in earlier training could be broken by repeated negative reinforcement.

III

Results

The results are summarized in Tables II and III. It will be noted that all the position-reward animals developed position habits within the 400-trial limit set by the research design. In all cases these habits were extinguished and symbol habits established within the 300-trial limit set for phase 3 of the procedure.

TABLE II

Results for the Position-Reward Group

	N = 9	
Phase 2:	Habit formed—9	Habit not formed—0
Phase 3:	Problem solved—9	

TABLE III

Results for the Position-"Frustration" Group

	N = 10		
Phase 2:	Stereotype formed—7		Stereotype not formed—3
Phase 3:	Problem solved—1	Problem not solved—6	Problem solved—3
Phase 4:		Problem solved—6	

Seven of the position-"frustration" animals developed stereotypes in phase 2, while three did not reach the criterion. All the latter established symbol habits in phase 3 and only one of the former. All six of the animals not developing symbol habits in phase 3 did establish them in phase 4.

Yates' exact test for 2 × 2 tables (Fisher, 1948) shows no significant differences between the two groups in phase 2, indicating that position responses were established as frequently under position-"frustration" as under position-reward conditions. On the other hand, a similar test of the data for phase 3 does show the existence of significant differences at better than the ·001 level of confidence. This may be interpreted as indicating that position stereotypes are more rigid—more resistant to extinction—than position habits, in which case the results support predictions from Maier's theory.

In phase 4 the punishment was increased to 100 per cent. Under these conditions all the six animals who had shown resistance to extinction in phase 3 did break their position stereotypes and developed symbol habits. This observation does not correspond with predictions from Maier's theory.

It is interesting to compare the number of trials taken by the two groups in extinguishing their position responses and developing symbol habits. The position-reward group took 138·33 (S.D. 72.58) trials under conditions of 50 per cent. punishment to reach the criterion for the symbol habit. The one animal of the position-"frustration" group establishing a symbol habit under these conditions of reinforcement reached the criterion in 202 trials, ·87 standard deviations above the mean of the position-reward group. The six animals developing symbol habits under 100 per cent. punishment took 99·17 (S.D. 31.53) trials to reach the criterion.

IV

Discussion

The results of this experiment show that position responses may be established under conditions of "frustration" as well as under conditions of escape training. However, the more interesting observations are those indicating a differential reaction of position responses established under these two sets of conditions to extinction during the development of a new response pattern.

Maier has suggested that this differential reaction reflects qualitative differences between "goal-motivated" and "frustration-instigated" behaviour patterns: ". . . abnormal behaviour fixations produced under frustration are different in kind from behaviours produced through motivated learning." Maier's results go further to suggest that these qualitative differences may reflect a differential effect of the conditions under which the position response is established on the *performance* of the response as compared to the *learning* of a new response. This is based upon the distinction between performance and learning made by Tolman (1932) in his learning theory and is supported by Maier's observations that, even though the position response appears, the animal's behaviour during a trial may give evidence that it is in fact able to differentiate the symbols presented.

Examination of the facts in the present experiment suggests two alternative hypotheses, either of which might account for the results obtained and both of which can be tested. One of these hypotheses involves the differential effects on extinction of practice under conditions of continuous and partial reinforcement. Jenkins and Stanley (1950) in their recent review of research findings on this subject present extensive evidence of significantly slower extinction following the development and maintenance of behaviour patterns with massed training under conditions of partial reinforcement. The development of position responses under conditions of the present research design is associated with almost continuous reinforcement for the position-reward group because of the small number of trials to establish the response in relation to the number of trials required by the criterion for establishment. On the other hand, the development of the position response in the position-"frustration" group occurs under conditions of partial reinforcement. In the present experiment, the actual per cent. of rewarded trials for the position-reward group was 98 per cent., and for the position-"frustration" group, only 49 per cent. On the basis of previous studies, it might be predicted that extinction would be slower in the latter group. This may have been the basis for the significantly different performance of the two groups in phase 3.

The second hypothesis arises from an examination of the pattern of reinforcement from the subject's point of view. In the case of the position-reward group a position response led to almost 100 per cent. reinforcement during the development of the position response. When the criterion of establishment had been reached and the experimenter had altered the nature of the problem, a position response led only to 50 per cent. reinforcement. The pattern of reward and punishment had been altered

significantly and, as the results indicate, the behaviour changed in relatively few trials—138·33. For the position-"frustration" group a position response led, during the development of the stereotyped response, to only 50 per cent. reinforcement. After the criterion of establishment had been reached and the problem altered from the experimenter's point of view, a position response still led to 50 per cent. reinforcement. The pattern of reward and punishment had not changed as far as the subject was concerned. As soon as this pattern was changed by imposing 100 per cent. punishment for position responses the position-"frustration" animals established the symbol habit in 99·17 trials. The possibility exists, therefore, that the initial position response was altered in one case as the results of a perceptible change in the pattern of reward and punishment, whereas no alteration of response occurred in the other case because no such change was perceived.

Neither of these hypotheses appears to be applicable to Maier's results, since many of his animals persisted in their position responses even after as many as 600 symbol-reward trials and after variation in the per cent. of reinforcement.

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(*Received 11th April*, 1951.)

THE PERCEPTION OF A CHANGING SHAPE*

BY

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Under specified conditions a pair of simple shapes are matched by a subject when almost all supplementary textural and space cues of depth vision have been removed. Under such conditions it is found that shape constancy is no longer present. However, the effect of regular rotatory motion of the shape is sufficient to restore constancy in the continued absence of other cues. Degree of perceptual constancy appears to be correlated with rate of change of shape. It is suggested that an explanation of the phenomenon is to be sought along the lines of Michotte's concept of "object creation" rather than in terms of gradient variables.

I

Introduction

THE present paper has been given this very general title but does not pretend to discuss the subject exhaustively. These observations are merely part of a more general investigation of shape perception at present being carried on by the writer, and it is hoped that it may be possible to publish a paper dealing with other aspects of this extensive subject.

Over a number of years many investigations of the appearance of a shape under various conditions have been made. In nearly all these studies the degree of perceptual constancy has in one way or another been made the measure of the "goodness" of the perception. That is to say, the degree to which the perception and the behaviour based upon it are more stable than their immediate objective conditions.

The earliest investigators presented alternately two shapes in a darkened room (Götz Martius, 1889) whilst members of the Gestalt School (Eissler, 1933; Klimpfinger, 1933) used shapes presented simultaneously, but separated by a large included angle. In the latter types of experiment comparison was made between one shape which is rotated about an axis and another which remains stationary. The shapes employed were usually circles, ellipses or squares such that the comparison could be approximated to the standard at some projective angle. During the actual rotation of the shape the subject is unable to see the objects, these being temporarily screened off. Stavrianos (1945) used a more elaborate technique in which these type comparisons were correlated with others in which the subjects had to estimate the real angle of orientation.

Thouless (1931) used a different method of comparison, his procedure being to present one shape as distant and foreshortened and the other as a projection of this, seen frontal-parallel and near. The near shape matched with the far was selected from a family of similar projections. For a full discussion of the history of experiments on shape perception the reader should consult the papers of Brunswik (1933), and Joynson (1949), on the problem of definition of shape. A general theoretical discussion of the problem can be found in Vernon (1937), Gibson (1950) and Koffka (1935).

From the standpoint of the present investigation all the various studies can be subsumed under a single heading, despite their theoretical disagreements, in that all are, in essence, investigations of the appearance of *stationary* shapes. Whether

* This research is part of work carried out at the Institute of Experimental Psychology and under a grant from the Medical Research Council, to which body, and Professor Humphrey, acknowledgment is made.

this is carried out by alternate inspection of two shapes or by simultaneous presentation, whether the comparison is made by rotating a shape to a judged match or by choice of a member of a family of shapes, in all cases the subject has before him a stationary shape which he may inspect for a relatively unlimited time. In addition to this, he is not able actually to see any process of change in the shape. Whether he has knowledge of how this takes place, or has any knowledge of the true character of the objects before him, and, if he has such knowledge, whether or not it influences his perception in the final outcome, might be speculated upon at some length (cf. "La Developpement de la Perception Visuelle," *Arch. Psychol.*, **93**, 1941).

What is clear is that the observer has no direct *perceptually given* knowledge as to this process of change. All that is before him is a certain fixed situation which he inspects and then, before the situation is changed for another one, makes such comparisons as appear to him reasonable. It is germane to the discussion to point out that in a number of experiments the results obtained when a subject was allowed to inspect and examine the set-up failed to show any significant differences from those in which judgments were made in the absence of such acquaintance. It would seem that intellectual, acquired knowledge does not influence perception in any directly measurable way. On the other hand knowledge of the situation acquired in the form of practice would appear to influence perception (cf. Klimpfinger, 1933).

At the same time there is definite evidence that variation in the rate at which a series of perceptual objects is presented can affect the outcome of the experiment. Thus Asch and Witkin (1948) reported that in experiments where a subject had to orientate a self-luminous rod to a judged vertical and/or horizontal setting, the presence of the "A" (Aubert) or "E" phenomenon could be noted under various different conditions, the rod being moved sometimes continuously and at others in a series of jumps. Under this last condition the amount of "oversetting" could be influenced by variation of the rate at which these jumps occurred. (The "A" and "E" phenomena are the errors made in perception of an oriented stimulus object when the subject's body is tilted. The amount and phase of error undergo sharp changes according to the extent of body tilt.)

The present writer has found in a similar way that, if the constancy experiments described above are performed without the interposition of a screen during adjustment of the shape, the constancy may be influenced by the rate at which alterations of shape are produced. And this change may be generalised as being in the direction of lowered constancy with slow or irregular changes and a rise in constancy when changes are regular and frequent.

Eissler and Klimpfinger in their experiments endeavoured to list a number of cues or determinants of shape perception and rate their importance. Among these were binocular vision, reflectance ratio, illumination and texture or microstructure. Apart from these "objective" determinants are also listed subjective or "dynamic" factors such as attitude of the subject ("pole of intention"—Brunswik) and the effect of practice. And it can be shown that the degree of constancy is progressively reduced by the removal of supplementary perceptual cues. But in the case of shapes rotated discontinuously, i.e. the subject being able to see movement taking place, the writer found that removal of the surface of the shape by substitution of a ring for a surface, did not entirely abolish perceptual constancy. Here, as in Boring's experiments on size constancy (Boring, 1941), the law of the visual angle was always exceeded even under the most unfavourable conditions. However, in this type of experiment there remains always one set of cues, namely, the appearance of the room in which the experiment is performed. This contributes the three-dimensional setting, the visual field in which the change of shape takes place. In

order to eliminate this last set of cues, the experiments were repeated in a darkened room using whitened wire outline shapes as test objects.

The procedure was to carry out the experiment with the room under normal illumination, the wire outline appearing as an abstract shape on a black ground. After obtaining matches under these conditions the room was then darkened and the shape set at the mean position given previously. The subjects were then asked if they agreed with their past judgments. If they did not (none did), the shape was brought to a new match point. The new judgments all centred about the point required for fulfilment of the law of visual angle. Thus it would seem that in the absence of supplementary cues from the object the normal visual setting had in some way influenced perception.

II

Aim of Experiments

It was decided to investigate the relationship between the perception of a stationary shape and a moving one. In normal and everyday perception objects are seen in movement. This movement is between objects relative to one another and the percipient, or in the case of stationary objects, due to the percipient's own movements. Moreover, ordinary judgments are made with some practical end in view and not as ends in themselves or as part of some scientific experiment. Thus the usual type of shape constancy experiment is, quite legitimately for its purposes, investigating a special class of perceptual phenomena somewhat removed from everyday experience (cf. Brunswik, 1933; Joynson, 1949).

It was thought that there might be some relation between constancy and the perception of movement, or *change* of shape. Gibson (1950) has suggested that two gradient variables for the perception of orientation and shape are the relative steepnesses, or slope, of (*a*) movement across the retina, and (*b*) the skewing of binocularly disparate images. Thus Gibson (p. 172) writes: "The steepness of a gradient towards crossed disparity running from the far to the near margin of a physical surface is proportional to the slant of that surface." If this is the case then it should be possible to isolate such variables and to measure their effect.

III

Method

Such an experiment requires that all the determinants to the perception of the real shape of the object must be controlled other than those it is intended to study. Thus all textural features, gradients of illumination, reflectance, etc., must be abolished, together with cues to distance and depth perception emanating from perceived walls, perspectives and so on, of the visual surroundings (Gibson, 1947).

In addition, the stimulus object must be simple and abstract, without internal angles which might assist deductions and help to give rise to analytic judgments based on the perspective of the rotating object itself. These requirements were met by making the object a simple circular outline of wire 1 mm. in thickness and 17 cm. in diameter which rotated on its vertical axis. Together with the circle was prepared a family of ellipse outlines separated by steps equivalent to 5° of rotation of a circle through its arc from 10° to 80°. It will thus be seen that there will be created 15 different ellipses as comparison shapes. The circle and an ellipse were set up 2·5 m. distant from the subject and were separated by a subtended angle of 10°. This included angle is sufficient to prevent the subject fixating both stimuli simultaneously.

The experiment is carried out in a darkroom which has been entirely covered with non-reflecting black paper and has internal traps to catch any stray light. The objects are seen by fluorescence, produced by coating the forms with a suitable substance and exposing them to U.V. radiation. Precautions were taken to avoid corneal fluorescence or any other undesirable effects from the light source. Isolated from the experiment is a control cabin equipped with means of varying the rate of rotation of the shape and noting its angular setting when the subject recorded a match. To indicate a match the subject presses a small push held in the hand. During the experiment the circle rotates from the line of regard through 90° and returns to its starting point, thus providing two matching points, one in each direction, waxing and waning.

The subject is seated comfortably in a chair and views the shapes at eye level, his head resting against the wall to his rear and one eye being covered with a shade. In judging the shapes he is asked not to make head movements. It was considered that any advantages which might accrue from the use of a head rest or biting board would be offset by loss of naturalness and ease for the subject. Subsequent test by the experimenter tended to show that small head movements did not result in any significant difference in scoring.

In order to compare judgments of shapes in motion with judgments of stationary shapes under identical conditions it was considered illegitimate to use shapes "set" behind a screen and then presented for inspection, since in the moving shape situation the subject is able to see all the different aspects of the changing shape and note that it has phenomenal identity and continuity. Therefore another apparatus was constructed whereby the subject was able to adjust the stimulus circle to a point of compared equality with the ellipse by remote control. For this purpose a "Selsyn" device was used so that a small knob could be placed in the subject's hand, and the judgment made and recorded in complete darkness. In this way the identity and continuity of the phenomenal shape was preserved whilst destroying the independence and regularity of the rate of change.

The method of scoring adopted was extremely simple, dispensing with the Thouless or Brunswik formula. Each velocity was tabulated as the only variable, judgments for all the different shapes being aggregated. The measure of constancy was simply the excess angle recorded over and above that required to satisfy the visual angle. Thus for the sum of all shapes, divided by the total number of shapes seen at any one velocity, the constancy value is comparable with similar values at other velocities. In order to measure the dispersions, the S.D. is calculated for each subject and the values averaged for each velocity.

IV

Results

TABLE I

Stationary Outline Shapes in Dark Space: Hand Set

Stimulus shape	35°	40°	45°	50°	55°
Match (in)	37°	41°	46·5°	51°	56°
" (out)	36°	41°	47°	51°	56°

S.D. for pooled mean (in) = 2·3 Mean constancy (in) C = 0·002
" " " " (out) = 1·8 " " (out) C = 0·002

Subjects 12. N = 340.

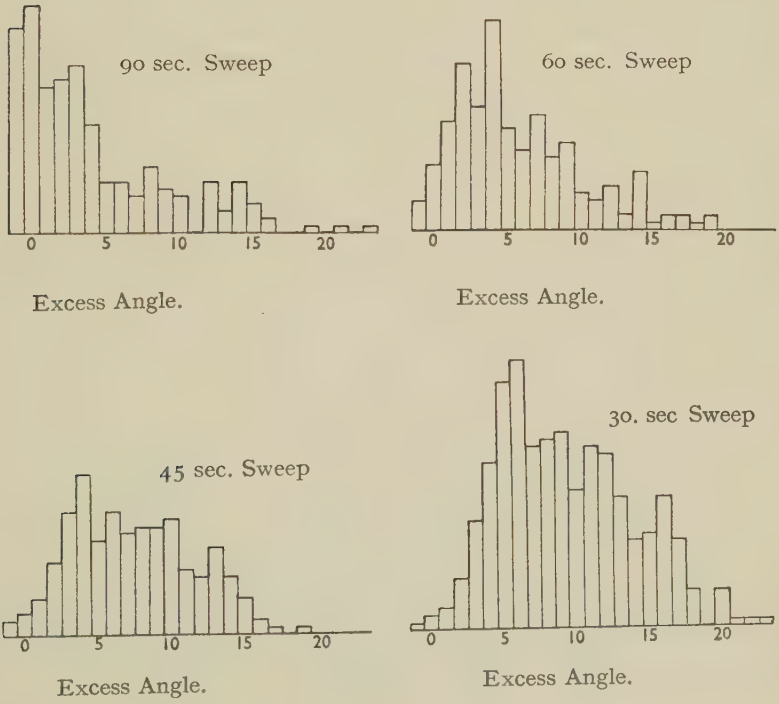
TABLE II

OUTLINE SHAPES IN DARK SPACE: MECHANICALLY MOVED AND MATCHED AT DIFFERENT VELOCITIES. VALUES AGGREGATED FOR ALL SHAPES

Velocity	90 sec.	75 sec.	60 sec.	45 sec.	30 sec.
Mean score	49°	49° 45″	50° 48″	52° 28″	54° 51″
Space error	1° 15″	1° 10″	1°	0° 52″	0° 55″
S.D. (in)	1·4	1·4	1·31	1·66	1·54
S.D. (out)	1·3	1·35	1·35	1·45	1·4
"V" (in)	2·86	2·81	2·58	3·16	2·8
"V" (out)	2·66	2·71	2·66	3·48	2·55
Constancy*	0·09	0·105	0·129	0·167	0·225

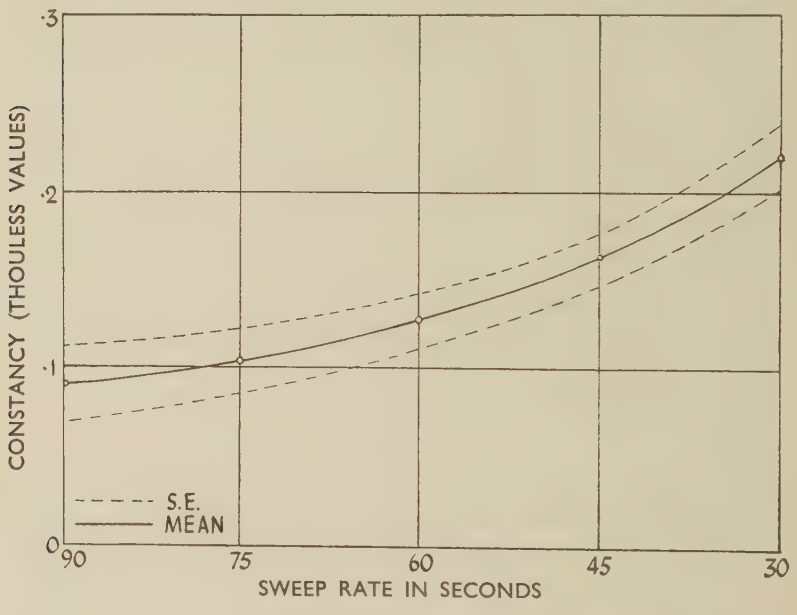
Subjects 12. N = 1140. * Mean Vis. Angle = 45°.

FREQUENCY HISTOGRAMS FOR FOUR VELOCITIES OF SWEEP RATE



From a comparison of Tables I and II it may be seen that there is a qualitative difference between the stationary and moving shape situations. Not only is the situation radically different even at the lowest velocity used but the rise in constancy with increase of velocity is continued as a smooth curve. In the case of the stationary, self-adjusted shapes the subject has relatively unlimited time to make a judgment, and it might therefore be argued that such a match is purely "analytic" in Brunswik's sense. At the same time, for the moving shape situation at the lowest velocity the subject has one and a half minutes during which the judgment may be made, yet the result suggests a totally different attitude. By attitude is meant a set of sensori-motor dispositions rather than a conscious mental state, since introspective comments volunteered by subjects in the course of the experiment tend to suggest that variations in the mode of appearance of the shape do not greatly influence their actual choice. Thus during the experiment it is common for subjects to state that at one moment they see a circle "turning round" and later to report that this appearance has been replaced by that of a circle being "squeezed in and out." However, examinations of scores made during these different experiences failed to reveal any significant differences. From the frequency histograms for four velocities it may be seen that there is a shift in the angles recorded in matches, from between $-1°$ and zero at the lowest velocity to a distribution approaching the normal around $7°$, at the highest. The rather wide distributions shown in the histogram are due not to variation in individual scores, as is shown by Table II, but arise from the nature of the constancy phenomenon.

Graph of Shape Matches with Continuous Movement of Stimulus



Each individual tends to have his own range of perceptual constancy; hence whilst means of pooled scores show large variations, grouping of scores, which ignores absolute magnitudes and records only changes in constancy with variation of some determinant, such as velocity of change, results in a smooth curve with small dispersions.

The histogram shows the whole range of possible scores, and it may be seen that at the lowest velocities the distribution of scores tends to be skewed sharply toward the zero (visual angle). It is obvious that there cannot be a normal distribution around the zero since this would correspond to the occurrence of "negative constancy."

It will be noted that the space error tends to fall with the increase in velocity. This error occurs, as might be expected, according to the direction in which the shape is rotating. Few subjects show anticipatory judgments, the general tendency being towards lateness in judgment. At a velocity higher than those covered in this experiment the space error becomes negligible. At this point, in the region of about 10 seconds per sweep, constancy reaches a level from which it shows no continued improvement. It would seem that around this velocity the perceptual situation reaches its greatest stability. It is also worth noting that it is in the region of higher velocities that subjects experience the reversals in the mode of appearance referred to above.

Whilst the range of velocities investigated were restricted to an upper limit of a 30-second sweep rate only by limitations in the available apparatus, it is nevertheless clear that there is an absolute upper limit to the velocity in this type of phenomenon above which the reactions of the subject will become hurried and confused. Such an experiment would then be investigating the speed and type of reaction by the subject rather than determinants of visual constancy. Consequently the range of experience to which these results apply must necessarily be limited.

In conclusion, the coefficient of variation has been calculated for Table II as it is misleading to compare the changes in dispersion by a simple measure since with increase in constancy the range of possible variation in match is also increasing. Twelve subjects, six of each sex, were used in the experiments, only two being students of psychology and five of the remainder university students. Two suffered from visual defects (corrected by spectacles), the rest having normal vision. A thirteenth subject was rejected as anomalous, but provided an interesting case by giving scores of negative constancy in Experiment I and zero scores in Experiment II. Subsequent study showed that this subject was unable to visualise the frontal-parallel ellipse as such in the dark space, but was compelled to experience it as a circle at some angle of tilt.

V

Discussion

It would seem that the class of changing shapes, in cases where the rate of change is directly perceptible, constitute a distinct class of shape appearances in which many of the conventionally assumed "cues" or "gradient variables" do not have the importance they can be demonstrated to have in so-called "normal" perception. In the case of stationary shapes it would seem that removal of gradients such as texture, surface, illumination, etc., together with the surrounding field or setting negates the perception of orientation, which together with outline makes possible constancy of shape.

In such situations the perception of motion, a change of shape occurring in a regular manner, introduces fresh variables of the type listed above, namely, sloped gradients of retinal sweep and the skewing of binocular images. But there would appear to be strong objections to accepting such gradient variables as sole explanation of the phenomenon. Whilst it may be true that gradients of retinal sweep contribute to a judgment of tilt, such a variable cannot of itself explain why perception of motion is ascribed to orientation. Why should not the shape be seen as being "squeezed in and out" rather than "turning round"? In addition, such

explanation leaves unaccounted for the fact that when this former experience does occur, constancy is not thereby affected. To make such an account acceptable it becomes necessary to invoke a learning process in order to "interpret" such a variable. But whilst putting forward a theory of gradient variables to account for constancy of perception, Gibson explicitly rejects a learning process as its concomitant, thus: "We do not have to learn that things are external, stable, rigid and spaced about the environment for these qualities may be traced to retinal images or to reciprocal visual-postural processes . . . the world does not seem to expand as we move forward in the environment although the retinal image does. The expansion is a stimulus correlate for moving forwards . . . the face of an object does not look compressed when seen at a slant since the compression is a correlate of its looking slanted" (Gibson, 1950, pp. 187, 190 ff.). This appears to answer the question by ignoring it, since there is no account of how these two sets of stimuli ever come to be correlated. Similar criticism of Gibson's views has come recently from Ittelson (1951), in the course of discussion of problems of size and distance perception.

The gradient of binocular disparity has even less explanatory value in the present case since the results in Table II were obtained using monocular vision; this variable can be at best only contributory.

Before passing to discuss an alternative line of explanation, it might be noted that in experiments of this kind a critical objection may be made, that what is responsible for the maintenance of constancy is the real, perceptual knowledge of the continuing identity of the circle possessed by the subject, and which he unconsciously "takes into account" when making a judgment. That this is not the case seems to be demonstrated by the results of Table I, where the subject also has perception of such a continuing identity, apparently without this knowledge affecting his immediate judgment.

The writer is indebted to Professor R. C. Oldfield for valuable suggestions as a result of which the following hypothesis is tentatively put forward.

In the present case not only have normally concomitant gradients of stimuli coming from the object itself been eliminated, but as far as possible those stimuli contributing to the perception of the surrounding field also. This would tend to prevent normal integration of object and field, or "figure and ground" into a perceptual whole, and under conditions of monocular vision there will be no true perception of depth, and hence no orientation or putative placement of the tilt of an object. In fact, as was suggested by the case of the thirteenth subject, there may even be illusory experiences equally legitimate to a true perception on the basis of the available data.

What seems clear is that the appearance of such a stimulus is purely phenomenal; there is no substantial "object," that is, a real subsisting entity behaving according to any general law. There is simply "an appearance." In the absence of supplementary cues a direct match between this phenomenal shape and another which is a projection of it will approximate to the retinal stimulation.

It is suggested that the intervention of motion in the manner of regular increase and decrease of the shape, taking place under limited ranges of speed, operates to "create" the object as a real and subsisting entity. In this way, analogously to the various perceptual phenomena demonstrated by Michotte, there is some interaction between the shape as a stimulus, and what is happening to it in a directly perceptible way, which gives rise to an object configuration, just as in the Michotte situations interation between perceptual "objects" give rise to "configurations of causality."

In a similar way, the "creation of an object" as described above may be noted

in the case of "Lissajous figures" seen on the screen of a cathode ray tube. When two frequencies are applied to the pairs of plates in such a tube it is possible to generate a closed figure, from the simplest case of an ellipse to complicated closed rings having a number of crests ("Crown" figures). As compared with the simpler figures the more complex ones possess, whilst stationary, some degree of apparent depth; yet here the intervention of motion can bring about a great increase in the perfection of the illusion. For if by means of a continuous phase shift the crests are allowed to progress around the ring figure, the whole configuration takes on the aspect of a three-dimensional object to an astonishing degree of reality. (Fisichelli 1947): (Weber 1930). And it is suggested that a shape constancy experiment utilising the outline figures produced by a cathode ray tube could be made to yield similar results to those discussed in the present paper.

In conclusion, it will be apparent that the type of explanation offered here does not claim to be an account of the way shape constancy actually operates. It would seem that a genuine account could come only from a full genetic study of the constancies in the course of individual development. In the absence of this it is only possible to speculate on the means by which the factors discussed above are integrated in the process of perception.

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LEARNING OF A SERIAL TASK BY DIFFERENT AGE GROUPS

BY

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In a serial learning task given to subjects whose ages ranged from twenty to seventy, a decline in performance showed itself as a loss of speed in the thirties, a loss of accuracy in the forties, and a marked loss of both speed and accuracy in the fifties and sixties. Experiments on recall and reorganization of material verified that older subjects were forgetting more quickly than younger, but that one of their main difficulties was an inability to substitute new reponses in an already mentally formed sequence.

Older subjects were not introducing new learning features but accentuating normal tendencies, such as the repetition of the same error at the same serial position. In so far as subjects learned their repeated errors, final learning involved unlearning, with the often observable phenomenon of the previously learned error serving as the cue for the more recently learned correct response. In terms of schematic concepts of mental functions, serial learning difficulty was not so much the formation of a general "scheme" but its subsequent amendment. Preformed habits influenced both the learning procedure and its results, particularly in the constraints and expectancies which the making of one serial response had upon another.

I

Introduction

This experiment had two aims; it was intended not only to investigate how the learning of a simple series was influenced by age, but also to see if an examination of the influences of the age variable had clarified some of the problems in learning studies.

The reason why these aims should be so interlinked might be mentioned here, since the point has some bearing on the relations between the "psychologies"—if not the psychologists—of the applied and the theoretical fields. It was anticipated that studies of ageing might throw some light on the essential and non-essential features of learning experiments, either by exaggerating normal tendencies or by introducing new ones It was the first of the alternatives which was so outstanding in this experiment; older subjects were emphasizing, generally by repetition, features which in some degree were common to all age ranges, and this very exaggeration made it easier to appreciate what were the outstanding difficulties in learning the series.

To illustrate how the interlinking of the two aims worked in practice, two examples of this exaggerated performance will be briefly indicated. The series to be learned was very simple in order that all older subjects could master it within the experimental period. Yet some subjects took so long to learn this short series that it was not only obvious when their progress was held up, but it became possible to study a process which could most fittingly be called the unlearning of previously learned responses. It was because of older subjects' extended efforts to learn in this situation where the number of possible responses was so limited, that the nature of this unlearning could be scrutinized.

The second example is given because it is related to recent work which has brought out the inadequacy of examining serial performances as if they were a succession of discrete stimulus-response units. Some of the experimental findings in such work have stressed that no matter how strictly the conditions are governed, factors outside

the experimenter's control are likely to influence the result. Whitfield (1950) illustrated these influences by proceeding to a logical end-point where the answers were given to imaginary questions. In the present paper an effort is made to indicate how frequently similar extraneous influences were actuating responses at some serial positions. Again it was a consideration forced upon the experimenter by the exaggerated performances of older subjects.

Thus the discussion in this paper, though conducted at a descriptive rather than an explanatory level, does aim to clarify some of the characteristics of serial learning, in addition to showing how their expression varies with age.

II

Experimental Method

Description of Apparatus.

The subject was presented with a row of ten lights positioned above ten morse keys, so that each light has a morse key just below and in front of it. Both the lights and keys are spaced two inches apart and are numbered one to ten.

A system of post office relays allows any series of lights, and any length of series within the ten lights to be set up. The key connections to the relays are also interchangeable so that any key can be made to control any one light. When a "correct" key is pressed it puts out the light which is showing and puts on the light which is serially connected to the last one. Every key is recorded separately.

Subjects.

Fifty subjects took part in the experiment, ten belonging to each decade from twenty to seventy. As far as possible in selecting subjects, those of each age group were matched for occupation and social background; seven of the ten represented business or the professions, and the other three were skilled or semi-skilled workers.

Experimental Instructions.

The apparatus was explained and demonstrated to a subject before telling him what he had to do. For the first part of the experiment he was asked to use only the keys 1–5. He had to find out and then learn a particular sequence of keys so that he could repeat it without a mistake. When this correct sequence was followed a light changed every time a key was pressed. Typewritten instructions were left with the subject; these were as follows:—

Instructions to Subjects

How the apparatus works.

When the "correct" key is pressed the light which is on goes out and another light comes on.

When an "error" key is pressed nothing happens.

In any one sequence one key will only put out one light.

What you are asked to do.

In this series you will only use the lights and keys 1–5.

The light will start at 1. You are asked to find the correct key which will put out this light and put it on at another point. You then find the next correct key for the light which has come on, and you repeat this procedure until the light comes back to its first position. This ends the first trial.

You rest for 15 seconds and then are asked to begin again, trying to remember and to hit only the correct keys.

You continue to repeat the sequence until you have learned the key series and can perform it without a mistake for two consecutive trials.

Design of Experiment

Stage I. Learning of Series I and II.

The subject learned a series of five keys, as instructed above. In series I the first five keys were used, in the order 2, 4, 3, 1, 5; in series II the other five keys in the order 7, 6, 10, 8, 9. Subjects were told that the order of keys in series II differed from that in series I. Both series were learned to a criterion of two consecutively correct trials.

Stage II. Relearning of Series.

After he had learned the second series the subject was asked to repeat the first series. If the recall was not correct at first, he continued until he had completed one correct trial. The second series was retested after the first.

Stage III. Alternation of Learned Series.

Finally the first lights in *both* series I and II were put on together. A subject was asked to begin with the first move from series I followed by the first move from series II, and continue the alternation between the series until he had completed both. Two trials per subject were made under these conditions.

For this stage subjects had to construct a new serial order out of the two previously learned sequences. A great deal of interference was anticipated in such a performance. It was hoped that the task would give some indication of the relative difficulty, and the nature of it, for different age groups.

Subjects never knew the full extent of the experiment until they had completed it, nor were they told that there would be any relearning or alternation tests until they were asked to do them.

The way in which certain terms will be used can conveniently be given here.

Trials.—A trial was measured from the time when the first light was switched on to when the subject pressed the last correct key in the series. (This returned the light to its original starting-point and after 15 seconds another trial began.) Thus in a single trial we had a measure of the time taken, and the errors in making the five correct responses.

Errors.—Any number of errors might have preceded the correct response at each position.

First errors.—Limits this number to the *first* error response only at each serial position, i.e. in a five-item series there could not be more than five such errors per trial.

Repeated errors.—A first error made at the identical serial position on more than one trial. This measure excludes the first time the error is made.

Serial position.—Serial position referring to the temporal order of a response within the series; and

Spatial position.—Spatial position referring to the actual place of the keys on the key-board are frequently contrasted with one another.

The three scores, trials, learning-time and errors are not independent. The number of trials is to some extent dependent upon the number of errors when the subject is learning to a criterion of two correct trials. Learning-time depends partly upon the number of trials and partly upon the general tempo of performance.

III

Results

A. Differences in Performance Between Age Groups.

Stage I.—The learning-times, the number of trials and the number of errors for both series are given in Table I and graphed in Figure 1.

The thirties made only slightly more errors and took slightly fewer trials than the twenties. Their overall learning-time was, however, greater than the twenties, because their tempo of performance as indicated by their time per trial was slower. This is further analysed in the section on learning times.

The forties took more trials and made significantly more errors than the thirties, but their learning-time was not proportionately greater—here accuracy had fallen off more than speed.

Above the forties all three scores, errors, time and trials increased rapidly. The sixties were taking more than twice the trials, making more than three times the number of errors and spending three to four times longer over learning the task than the twenties.

TABLE I

LEARNING-TIME, NUMBER OF TRIALS AND ERRORS FOR AGE GROUPS IN SERIES I AND II

Age group (10 *subjects per group*)	*Trials*		*Time*		*Errors*	
	Means (*in secs.*)	*S.D.*	*Means* (*in secs.*)	*S.D.*	*Means* (*in secs.*)	*S.D.*
			Series I			
20–29	6·0	2·32	71·8	21·10	16·7	7·34
30–39	4·9	2·00	87·0	22·29	18·4	14·48
40–49	5·9	2·45	85·4	22·23	23·1	9·79
50–59	10·4	4·59	193·1	50·89	39·5	25·10
60–69	12·3	5·91	238·3	93·48	53·9	37·30
			Series II			
20–29	3·9	1·64	42·4	13·03	11·8	11·98
30–39	3·9	2·73	60·6	21·26	11·9	7·30
40–49	6·2	3·40	81·0	22·67	24·8	16·10
50–59	8·4	4·69	155·1	60·89	29·8	22·41
60–69	9·6	6·16	207·1	74·99	46·6	41·25
			Totals for Both Series			
20–29	9·9	2·38	114·2	22·62	28·5	15·33
30–39	8·8	4·31	147·6	41·95	30·3	18·40
40–49	12·1	5·11	166·4	34·59	47·9	21·82
50–59	18·8	6·85	348·2	92·07	69·3	44·43
60–69	21·9	11·30	445·4	155·30	100·5	69·91

All figures are up to and including first correct criterion trial. The normal deviate in all the tables has been calculated in accordance with the formulae given by Kendall (1948) for the rank correlation coefficient.

Errors: Ranking all subjects by age ranges.
Normal deviate = 4·20, $P < 0.001$.
Dichotomy: Subjects in thirties and forties.
Normal deviate = 2·08, $P < 0.05$.

Time: Ranking all subjects by age ranges.
Normal deviate = 4·48, $P < 0.001$.
Dichotomy: Subjects in forties and fifties.
Normal deviate = 2·46, $P < 0.02$.

Time per trial: Dichotomy: Subjects in twenties and thirties.
Normal deviate = 2·23, $P < 0.05$.

The variability of performance increased greatly with age, as indicated by the rise in the standard deviations for all three scores. In so far as these standard deviations are roughly proportional to the corresponding means, it can be said that the relative variability of the different decades is not greatly increased But in absolute measure the variation between subjects in the higher age ranges is very much greater than in the lower, and this produces some overlap in scores between individuals of different age groups. That this is not marked can be seen by the highly significant figures when the errors or the times of all subjects are ranked by age groups.

Stage II: Relearning.—The relearning data are given in Table II.

The decline in performance is similar to that in the original learning except that the thirties are now significantly less accurate as well as slower than the twenties.

FIGURE I

MEAN TIMES, ERRORS, AND TRIALS PER SUBJECT FOR LEARNING BOTH SERIES (excluding criterion trials)

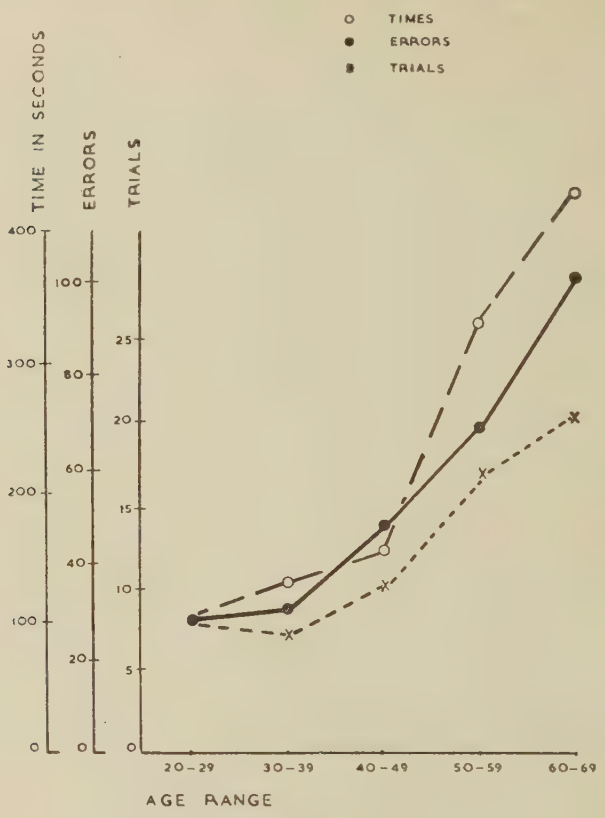

TABLE II

RELEARNING TEST FOR BOTH SERIES

Age groups (10 *subjects per group*)	*Total Relearning time* (*in secs.*)	*Trials*		*Total errors*	*How often series was correctly recalled on first trial.* (*Maximum possible* = 20)
		Total	*Total incorrect trials.*		
20–29	115	23	3	7	18
30–39	333	44	24	40	12
40–49	376	42	22	77	9
50–59	842	56	36	119	4
60–69	878	66	46	166	2

Total figures include first correct criterion trial.

Errors: Ranking all subjects by age ranges.
Normal deviate = 4·83, $P < 0{\cdot}001$.
Dichotomy: Subjects in twenties and thirties.
Normal deviate = 2·38, $P < 0{\cdot}02$.

Time: Ranking all subjects by age ranges.
Normal deviate = 4·56, $P < 0{\cdot}001$.

Results at this stage depend mainly on recall, as impaired by the length of time since learning a series, and also by the retro-active effects of subsequent learning. This decline of the thirties stresses that, however comparable their learning was to the twenties, their forgetting was certainly quicker.

The first errors which were made at the relearning stage were mostly repeats of the first errors which the subject had made during the initial learning. This was verified by checking the agreement between the modal error which each subject made for each serial position at the two stages of the experiment. Of the total number of modal errors in the relearning, 77·1 per cent. agreed with the subjects' modal error in the learning.

Stage III: Alternation of Series.—The data for the third stage of the task (alternation of items from the two previously learned series) are given in Table III. Aggregate

TABLE III

ALTERNATION OF SERIES
(two trials only)

Age group (10 subjects per group)	*Total time (in secs.)*	*Errors*	*Number of correct trials (20 possible for each age group)*	*Times per key struck*
20–29	489	28	9	2·14
30–39	484	55	7	1·90
40–49	594	75	5	2·16
50–59	763	77	2	2·72
60–69	1,179	152	0	3·35

Errors: Ranking all subjects by age ranges.
Normal deviate = 4·07, $P < 0.001$.

Time: Ranking all subjects by age ranges.
Normal deviate = 5·01, $P < 0.001$.

time and errors are shown and as these are for the same two trials, the time per key struck is also given. Here, as in the initial learning the tempo of older subjects is markedly slower. The fifties and sixties found this a very difficult task—only two out of their forty trials were correct. In alternating the two learned series a subject had not only to select and differentiate between items in a manner apparently akin to that required in the original learning; there was a further difficulty, which the older subjects in particular experienced, and which could be mainly ascribed to two causes: firstly, the stress which this selection imposed upon memory. The subject had to retain in two separate series the responses he had just made, and at the same time recall the serial responses which were to follow; and secondly, negative transfer arising because a response no longer followed in its previously learned serial order, but was replaced by a new response.

B. Increase in the Learning-time with Age.

Subjects had been given no instructions whether learning-time or errors were being scored. From the nature of the task it is likely that they would have been led to pay more regard to accuracy than to speed, since there was no indication at what speed they should perform, but there was immediate indication whether their response was right or wrong. So far as could be judged both on these grounds, and from observations by the experimenter, subjects were setting their own tempo

of performance, i.e. the time taken to strike each key. The variations with age in preferred tempo can be illustrated by considering "time per key struck" on the first trials only, during the initial learning of series I and II.

TABLE IV

TIMES FOR FIRST TRIALS ONLY

Age group (10 *subjects*) *per group*)	*Series I*			*Series II*		
	Time per first trial (*in secs.*)	*Time per key struck* (*in secs.*)	*First trial time as per cent. of total learning time*	*Time per first trial* (*in secs.*)	*Time per key struck* (*in secs.*)	*First trial time as per cent. of total learning time*
20–29	170	1·43	22·3	177	1·53	39·2
30–39	290	2·36	31·5	212	1·78	32·7
40–49	260	2·04	29·0	232	1·86	27·6
50–59	338	2·40	16·9	269	1·95	16·7
60–69	339	2·40	14·0	311	2·22	14·7

Decrease in learning-time between series I and II.

Subjects in the twenties and thirties: Difference between the two means for the decrease of time between series I and II: $t = 3{\cdot}022$, $P < 0{\cdot}01$.

It will also be seen from Table IV that there was a striking contrast between the performance time for the twenties and the thirties. For the first trials in series I the twenties took 170 seconds and the thirties 290 seconds; for the first trials of series II the twenties were the only age group to increase their time while the thirties decreased theirs from 290 to 212 seconds. Taking as the variable the decrease in time from series I to series II the mean value for the twenties was found to differ significantly from that of the thirties.

These facts suggest that from their experience of performing the first task the younger subjects recognized that their initial speed was not advantageous to their learning. Support is given by the direct observation of the procedure which these subjects adopted, and by their comments whilst carrying out the task. At the first trial they were willing to begin without any clear idea of how they were going to proceed, and allowed the actual performance to dictate their technique. As a result of the practice effect from series I they completely redistributed their learning-time in series II, spending 39 per cent. of their total time on the first trial.

The thirty to fifty age groups were much more painstaking in their approach, carefully examining display and controls both before and during the first trial. They asked many leading questions such as, "Then I don't need to look at . . ." etc. They were seeking answers not only about the apparatus but how they were to perform on it, so that they tended to start with some method for doing the task. The effect of practice for the thirties, and for all the age groups except the twenties, was that they began the second series at a quicker rate than the first, but it is of interest that in all these age groups the ratio of the first trial time to the total learning-time is constant over the two series. (See Table IV, percentage column figures). Only the twenties increase this time distribution on the second series.

From the figures in Table IV indicating that for the fifties and sixties the "time per key struck" is about one and a half times that of the twenties, it appears that

the slower tempo of performance of the older subjects was contributing a little to their longer overall performance time. But this slower tempo would not account for the whole of their increased learning-time which is three to four times that of the twenties. A study of the type, the number and the repetition of errors gives some explanation why the time increase was so great.

It is being stressed that this increase may have arisen from more than one contributory cause, since there has been a tendency in ageing studies to measure overall time and contrast it with the quality of performance. Sward (1945), for example, says, "Other things equal, age has the effect of impairing the rate far more than the quality or the accuracy of the mental operations here considered."

It may well be that scores of accuracy on such tests between different age groups are often more comparable than their overall time scores, leading to conclusions such as the above. But the different time scores between age ranges may be indicative of just those changes in "the quality of the mental operations" which their identical scores for accuracy are concealing; that is to say different age groups may have gained the same end score of accuracy by very different "mental operations." There is a hint of this in the same error results but different time scores for the twenties and thirties.

C. Characteristic Errors in Learning this Serial Task.

So far we have considered the methods by which this task was executed only in respect of quantitative criteria—number of trials, number of errors and time taken. In this section we shall be more concerned with how performance was qualitatively modified by certain characteristic errors of serial learning. We shall be considering two types of learning mistakes—those arising from various expectancies which subjects had about the *spatial* randomization of responses, and those occurring because subjects anticipated items in the partly learned serial order.

(1) *A Spatial Expectancy Error.*—The errors in series I and II do not conform to the usual distribution within a series where the maximum point is at the centre. It will be seen from Figure II that at some points in the two series there is a striking reversal in the distribution of errors; for example, the second position has the minimum number of errors in the first series and the maximum in the second. In order to examine how far this tendency had persisted throughout the course of learning the *first* trial of all 50 subjects was analysed; the number of errors made at each serial position is given in Table V. The distribution of errors in the first trial, particularly that on series II, is similar to that for the total learning of the series; the second position still has a strikingly different number of errors for the two series.

We cannot account for this result by any specific interaction between series I and II; the maximum number of errors still occurred on the second serial position when series II was given independently. The explanation seems to be that a subject brings to the learning of a serial task certain expectancies and these are of a general kind, independent of a specific transfer from a previously executed series. The most obvious illustration of this is the subject's idea of alternation. When he has hit a key at one end of the board he expects the next response to be towards the other end. He does not anticipate that the next key may be adjacent to the one he has just struck. As one subject put it: "I was working on the principle that if I hit a correct key at one end, then I tried the other."

It is of some note that, coupled with this tendency, subjects seemed to find it easier when the movement involved going from left-to-right than from right-to-left.

If we check the number of errors made in learning keys which involved these contrasted movements, we find the errors scored were 946 from left-to-right and 1,506 from right-to-left. However, as these movements could not be equated in their serial positions, Table VI only gives the figures for the second and third serial positions.

FIGURE II

Total Error Responses at each Serial Position in Learning Series I and II (50 subjects)

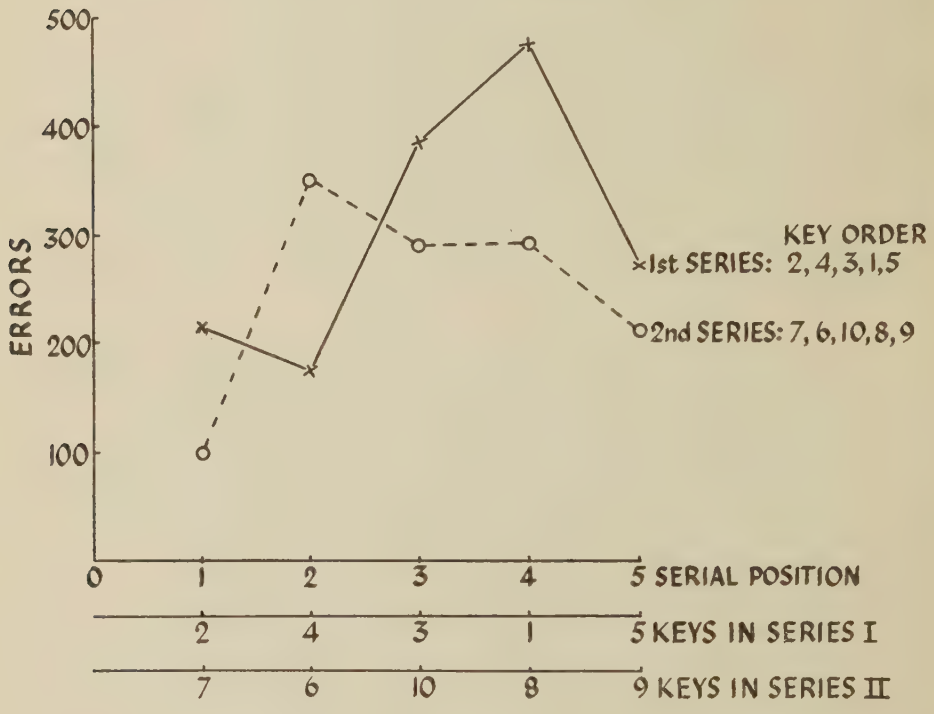
500
400
300
200
100
ERRORS
KEY ORDER
1st SERIES: 2, 4, 3, 1, 5
2nd SERIES: 7, 6, 10, 8, 9
0 1 2 3 4 5 SERIAL POSITION
2 4 3 1 5 KEYS IN SERIES I
7 6 10 8 9 KEYS IN SERIES II

TABLE V

Error Responses at each Serial Position on First Trial

	Series I					*Series II*				
Serial position	1	2	3	4	5	1	2	3	4	5
Correct key	2	4	3	1	5	7	6	10	8	9
Number of errors ..	60	73	92	79	94	56	128	93	68	51

Examples such as these two—the subject's idea of randomness and the left-to-right contrasted with the right-to-left movement—suggest two points. Such features emphasize that in considering the effect of serial position upon learning, it is necessary to allow for the influence of more than the ordinal position of the item within the series. The idea of serial position needs to include the probably more fundamental point of the constraints and expectancies which the making of one response will have upon the next—one action is giving the "direction" or cues

for the next. It is in its relational context both to previous and to anticipated responses that the ease or difficulty of learning an individual response must be understood.

At the same time it is being stressed that these specific predilections for certain serial items or for directions of movements merely exemplify more generalised tendencies, and they have been put forward to illustrate the more general point. It seems that when subjects have to make a choice at any particular serial position they do not give equal weighting to all possible responses. They bring to the task, as illustrated by data from their first trial, definite preconceptions as to what it would demand of them, and how they would comply with these requirements. And their responses throughout learning show that, far from having only an initial influence, such preconceptions have a lasting effect. These then are variables which are inherent in adult learning and in so far as it is probable that the older subjects have the more definite preconceptions their learning is the more likely to come under such influences.

TABLE VI

ERRORS MADE IN LEARNING LEFT-RIGHT AND RIGHT-LEFT MOVEMENTS

	Left to right errors	*Right to left errors*
Second serial position	176 (key 4)	350 (key 6)
Third serial position ..	290 (key 10)	385 (key 3)
Totals	466	735

(2) *Anticipatory Errors.* These errors—"one of the very widespread phenomena of learning" (McGeogh, 1942)—have been discussed by learning theorists of all schools. (Spragg, 1936, briefly reviews the literature on the subject.) Normally the name includes all responses which are subsequently correct in the series. Here, because the series is short, we are only considering the immediate anticipatory error, i.e. the first error response made at any serial position. In calling these errors anticipatory the intention is to emphasize their relation to responses which are subsequently correct in the series; it is implied that the subject is aware of some serial relationship between the responses which have been made and those which are still to be made.

We shall consider in this section the part which anticipatory errors are playing as learning proceeds. Of all errors they were the most common and the most persistent. This is illustrated by the learning record for a single subject, shown in Table VII. It will be seen that in series I, where the correct order was 1, 5, key 5 was repeatedly hit at the serial position of key 1; in series II where the order was 8, 9, key 9 was hit at the serial position for key 8. In order to bring out the generality and persistency of this type of mistake, the total distribution of first errors is shown in Table VIII. The error keys are placed in serial order so that the immediate anticipatory error is shown first at each serial position. In nearly every instance most mistakes were made at this first point; the one exception was the first serial key in series I. The distribution over all the keys was anything but random; there was a definite tendency for those responses which were serially nearer to the correct response to occur more frequently.

In an attempt to find out how this process of concentrating upon the anticipatory errors developed during the course of learning, the total distribution of Table VIII

TABLE VII. INDIVIDUAL SUBJECT'S LEARNING RECORD FOR SERIES I AND II

	Series I					*Series II*				
Serial Order of Keys	2	4	3	1	5	7	6	10	8	9
TRIALS 1	—	3	5	—	2_3	—	$8_{7_{9_7}}$	9_{7_8}	6_{7_9}	$7_{6_{10}}$
2	1_3	—	—	$5_{4_{3_2}}$	3_{2_4}	9_8	$9_{8_{10}}$	9_{8_7}	6_{7_9}	7_{6_8}
3	1_{3_4}	5_3	2	$5_{4_{3_2}}$	4	—	$9_{8_{10}}$	9_{8_7}	9_{7_6}	$7_{6_{10}}$
4	1_4	3_5	2	5_{4_2}	3_4	—	$9_{8_{10}}$	9	—	$7_{6_{10}}$
5	1_4	—	5	$5_{4_{2_3}}$	—	—	$8_{9_{10_{9_8}}}$	8	—	6
6	1_{3_4}	—	—	$5_{4_{2_3}}$	—	—	9_8	—	9	6
7	1	—	—	$5_{4_{3_2}}$	—	—	$9_{8_{10_9}}$	—	—	—
8	—	—	—	5_{2_4}	4	—	—	—	9	$7_{6_{10}}$
9	—	—	—	5	4	—	—	9	9	7
10	—	—	5	4_{2_5}	—	—	9	—	9_7	—
11	—	—	—	5	—	—	$10_{9_{8_9}}$	—	7_9	$7_{6_{10}}$
12	—	—	—	5	—	—	$9_{8_{9_{10}}}$	—	9	—
13	—	—	—	—	—	—	10_{9_8}	—	9	—
14	—	—	—	$5_{4_{3_2}}$	—	—	$10_{9_{10}}$	9	—	10
15	—	—	—	5	—	—	$10_{9_{8_{10_{8_7}}}}$	—	9	—
16	—	—	—	5	—	—	$10_{9_{10_8}}$	—	—	—
17	—	—	—	5	—	—	9_{10_8}	9_8	9	—
18	—	—	—	4_5	—	—	9_{10}	—	9	—
19	—	—	—	—	—	—	10_9	—	9	—
20	—	—	—	—	—	—	—	—	9	—
21	—	—	—	—	—	—	—	—	9	—
22	—	—	—	—	—	—	—	—	—	—
23	—	—	—	—	—	—	—	—	—	—
Total errors at each serial position	13	5	5	41	11	2	56	15	24	22
Total 1st errors at each serial position	6	3	5	16	7	1	17	8	16	10

was further analysed. Table IX, laid out in the same manner as Table VIII gives the percentage number of errors at each serial position for three different stages during the course of learning—the beginning, the middle and the end of each individual's learning record for each particular serial position. If we compare these figures with the percentage figures for Table VIII we can see that initially (row A) there was little or no tendency to agree with the anticipatory order: we have an almost random distribution. By the middle stage of learning (row B) the distribution of errors was conforming to the serial order, and this tendency to concentrate errors more and more upon the key which would be correct at the next serial position was at a maximum in the last stages of learning (row C). The process was not one which developed and then declined, rather it was something which developed and was most rigid in the later stages of learning. A subject's error responses were the most predictable at the point just before learning was completed. These results imply that the length of time which had elapsed, and the number of events which had taken place, since a key was correctly struck, partly determined how often that key was tried at any serial position. From observing subjects' performances when these anticipatory errors occurred it was often possible to distinguish one of two procedures being followed. (It is not implied that these were always distinct, and indeed it is thought that the second succeeded the first as learning proceeded.)—

(1) The first procedure, which characterised the early stages of learning, was generally slow and hesitant as if a subject, not knowing the right key, pressed one which he had not used recently.

(2) The second was a more definite selection and typical of the later stages of learning, when the subject had established some serial order and was anticipating responses which he knew he had yet to make. A key was selected because it was thought to be the next serial response and in fact it proved to be the next but one. The speed with which this wrong response was made, and a subject's surprised reaction when he discovered that it was wrong were indicative of this stage of learning. This point is considered more generally in the discussion.

C. Repetition of Errors.

The discussion of anticipatory and spatial expectancy errors, giving examples of the disproportionate frequency with which certain keys have been hit at some serial positions, provides some preliminary indication how far subjects will continue to repeat the same mistakes. It should be stressed that errors and error repetition were not differently distributed between age groups, but that older subjects were exaggerating repetitive tendencies which were typical of all age groups. In the fifties and sixties 76·3 per cent. of their *first* responses for each serial position were repeats of former errors. Some repetition is, of course, inevitable in a five-item serial task. Even so, an examination of individual learning records such as the one given in Table VII, or of the distribution of errors as shown in Table VIII, makes it fairly clear that this repetition is not due to any chance selection. There is an obvious tendency to repeat some errors at the same serial position in successive trials.

When an attempt is made to demonstrate this preference statistically the difficulty arises of assessing the chance probability of a particular key being struck at a particular point. This probability almost certainly changes during the course of learning when some of the responses are learned more quickly than others. A binomial method was adopted of showing for one serial position a marked concentration upon one error key as opposed to all the other error keys, which up to that point

TABLE VIII

TOTAL DISTRIBUTION OF FIRST ERRORS AT EACH SERIAL POSITION
(for all subjects)

Series I

Correct key (in serial order) ..	II				IV				III				I				V			
Error keys (in serial order) ..	4	3	1	5	3	1	5	2	1	5	2	4	5	2	4	3	2	4	3	1
No. of errors ..	11	31	63	20	78	24	16	3	84	62	34	8	145	49	43	—	71	23	22	4
Percentage distribution of errors for each serial position. . ..	8·8	24·8	50·4	16·0	64·5	19·7	13·3	2·5	44·7	33·0	18·0	4·3	61·2	20·6	18·2	—	59·3	19·1	18·3	3·3

Series II

Correct key (in serial order) ..	VII				VI				X				VIII				IX			
Error keys (in serial order) ..	6	10	8	9	10	8	9	7	8	9	7	6	9	7	6	10	7	6	10	8
No. of errors ..	24	7	21	8	62	29	52	1	71	52	27	4	70	49	38	—	39	35	32	5
Percentage distribution of errors for each serial position ..	40	11·7	35	13·3	43·0	20·1	36·1	0·8	46·1	33·8	17·5	2·6	44·6	31·2	24·2	—	35·2	31·5	28·8	4·5

The correct and error keys are in their serial order so that the distribution of errors according to this order becomes apparent. The first error key (i.e. the immediate anticipatory error which is underlined) is the key which will be correct at the next serial position; the last error key is the one which has just been correct at the last serial position.

TABLE IX

DISTRIBUTION OF FIRST ERRORS AT THREE STAGES DURING LEARNING
(At each serial position the figures give the *percentage* number of errors on each key)

Series I

Correct key (in serial order) ..	II				IV				III				I				V			
Error keys (in serial order) ..	4	3	1	5	3	1	5	2	1	5	2	4	5	2	4	3	2	4	3	1
(A) Errors on first trial	11·1	13·9	58·3	16·7	66·5	26·1	4·8	2·4	24·4	53·3	15·6	6·7	25·0	33·3	41·0	—	54·0	10·9	35·1	—
(B)* Median errors	7·7	23·1	46·1	23·1	58·9	23·5	17·6	—	56·0	28·0	16·0	—	60·6	21·2	18·2	—	55·6	22·2	22·2	—
(C)† Last errors ..	17·6	29·4	47·1	5·9	70·8	16·7	8·3	4·2	58·9	26·4	11·7	3·0	82·5	12·5	5·0	—	63·1	36·9	—	—

Series II

Correct key (in serial order) ..	VII				VI				X				VIII				IX			
Error Keys (in serial order) ..	6	10	8	9	10	8	9	7	8	9	7	6	9	7	6	10	7	6	10	8
(A) Errors on first trial	34·4	21·8	34·4	9·4	14·6	29·2	53·7	2·5	43·2	2·5	25·0	6·8	35·3	20·6	44·1	—	16·7	58·3	25·0	—
(B)* Median errors	43·0	14·0	43·0	—	43·7	25·0	31·3	—	45·0	25·0	30·0	—	33·3	52·4	14·3	—	40·0	40·0	20·0	—
(C)† Last errors ..	43·0	14·2	42·8	—	50·0	29·0	21·0		57·1	35·7	7·2	—	50·0	35·3	14·7	—	45·5	27·3	22·7	4·5

* Row B was plotted by considering each serial position separately for each subject. The median error was found for each serial position and the error key at that point was scored. Figures were summated for all subjects and the percentage distribution of errors at any one serial position was obtained.

† Row C followed a similar procedure to plot where each subject made his last error at each serial position.

in the series had not been correctly struck. The second serial position in series I was taken as an example: the correct key is No. 4 and the three error keys are 3, 1 and 5. The calculation was made on the assumption that all three error keys had an equal chance of being struck. When the probability for each individual subject in the fifty and sixty age groups was summated, it was found that key 3 as compared with the other error keys had been chosen significantly more often than chance. Using the method given by Fisher [1925, p. 99] of combining the probabilities, values here were $\chi^2 = 61{\cdot}55$, degrees of freedom $= 40$, $P < 0{\cdot}02$.

This demonstrated that there was discrimination upon one particular error response as opposed to another. An attempt was then made to show that a similar preference was operating when the choice lay between two responses which had not yet been correctly made in the series, but when one was an error and the other the correct response. The fourth serial position leaves the subject with two such responses: in series I this would be the anticipatory error, key 5, and the correct response, key 1. It will be remembered that the learning criterion was fulfilled when two consecutively correct trials were completed. But the difficulty arises that this serial position was generally the last to be learned, so that the striking of the right key at this position for two successive trials would have fulfilled the learning criterion, even if such responses were accidental. Thus, the number of incorrect attempts observed was almost certain to be higher than the number of correct attempts. The problem was solved in an indirect way. It was assumed that there was equal probability of a subject hitting either the correct or the error key (i.e. $P = \frac{1}{2}$ at each trial) and the chance of continuing for any given number of trials without hitting the right key twice in succession was calculated. It will be appreciated this was a strict criterion—it assumed no learning of the correct response by the subject. Even so, when the individual probabilities for the subjects in the fifty and sixty age groups were summated, it was found that the probability of obtaining by chance as many trials at this serial position as those actually observed was very small. The expected number of trials by chance was 6. The mean observed number of trials at this point was 8·9. Thus it can be said that during learning these subjects had shown a significant preference for the error rather than the correct key. ($\chi^2 = 60{\cdot}55$, degrees of freedom $= 40$, $P < 0{\cdot}02$.)*

This gives some statistical verification that a subject is choosing to make a particular error at a particular serial position in preference to both other error and correct responses. Of course, this is no new phenomenon, but can be substantiated by the results of other serial learning experiments. Lumley (1932), for instance, gives examples of increasing rigidity of responses as learning proceeds, as do McGeogh (1942) and Spragg (1936). It is only in the unusually persistent repetition of the older subjects that anything novel was introduced.

IV

Discussion

In so far then as these results are exaggerations of typical learning features observed by many experimenters, they can serve as the basis for the following observations.

Influence of Preformed Habits.

The experimental findings indicate that for all subjects there is a distinction in learning between the difficulty of retaining the correct items, and the difficulty

* For this method and for much assistance in discussing the problem, the writer is indebted to Miss Violet Cane.

of retaining items some of which may not be correct. The question why a subject should find it easier to learn certain error responses rather than their correct equivalents is as difficult as it should be revealing. Older subjects were certainly capable of establishing, relatively quickly, a series of consistent responses; in a comparatively short period they were retaining some facts. Differences in final learning-speeds between age groups were not to be explained completely by differences in rate of forgetting.

The results presented in the section on spatial errors suggest that one explanation why some items were learned more quickly than others was to be found in the preformed habits of subjects. This can be related to Bartlett's work on dominant detail where dominance seems so often to be established as a result of experience (Barlett, 1932). Subjects learned those items to which their preconceptions of the material and their general expectancies had directed their attention. Ageing studies, in their emphasis upon how far subjects attempt to relate instructions to their own experiences, do stress that a learning experiment and its material cannot be treated as isolated phenomena with independent and measurable objective difficulties.

These findings can be placed in the general context of experimental evidence illustrating how far subjects have their own preconceptions by which they make for themselves a structured field however unstructured it may seem to the experimenter. Of such work Skinner's (1942) analysis of the repeated guessing of alternatives, and Whitfield's (1950) "The Imaginary Questionnaire" are some of the clearest examples of the initial tendencies which subjects bring to a situation. The data of the present experiment show how these initial tendencies continue to influence subsequent learning, both in the manner of performance, no less than in the result.

Interaction of Items in Serial Learning.

Rote-learning of a series seemed to be a positional fitting together in a unified relationship of a number of originally discrete items. In studying how a subject assembled this material we have to allow for the interplay between items as they were combined within a total pattern. Every item became related to every other item, and was influencing its choice at any particular point. We have noted this influence at the beginning of learning where one response indicated, even dictated, the choice of the next, so that the ease of learning the next item depended upon how far its position conformed with such indications. Later, as learning proceeded there was a tendency for the subject, when he reached an unkown point in the series to put forward the learned items which were closely related to the unknown point. Generally, as we have seen, the subject did not repeat the correct responses he had just made, but selected responses which followed the unlearned item in the series —that is, he made anticipatory errors. The result was that the subject established a mental series which was shorter than the actual series, because two known items were related together to the exclusion of an unknown. If learning was to be completed this relationship had to be amended by adding to or substituting within the originally learned sequence. But the older subjects, having achieved some serial organization of material, tended to adhere to it.

It was this correction, demanding substitution within the sequence which was one of the weakest parts of older subjects' learning. Once incorrect responses had been learned, the subject had to learn new responses to replace them, whilst repeating the old responses which were correct. The learning of a new correct response at a previously learned serial point—here equivalent to the old stimulus—puts the learner in a negative transfer situation. The point emphasizes how far rote-learning

does involve a correction of errors, and the essential difficulty of that correction when it necessitates a modification to the mental form which a learner had given to a series.

The Need for Amending Schematic Material during Learning.

If this is thought of in terms of some schematic concept of mental functions, the crucial learning difficulty was not so much the formation of a general "scheme," but the amendment of it. What the subject had schematically determined to be the response was predicted as the response, and with some subjects continued to be so predicted long after objective evidence had repeatedly shown it to be otherwise. It was in this sense that a subject could be thought of as trying to impose a schematic form upon the material, and being reluctant to amend it. It was a reluctancy that was obviously increasing with age.

From the point of view of a subject, it can be said that in rote-learning he introduced and, in so far as he repeated them, learned his own errors. Failure to modify the learned series led to continual repetition of the same responses. It was the paradox of this repetition that, whilst it strengthened the correct responses, it often seemed to be reinforcing the errors, which as we have seen, became more stereotyped and rigid. This was almost a vicious circle procedure, in that, if a subject was left to his own resources, he could only learn by further repetition, but that repetition was also strengthening the errors he was trying to eliminate.

The continual repetition of errors and the difficulty of their correction are, of course, not peculiar to rote-learning. Stereotyped procedures, which would parallel the continual repetition of errors have been frequently found in problem-solving and might be compared to the rigidity of behaviour reported by the psychiatrist The previous section tried to bring out how this difficulty of correcting mistakes in a learned sequence was a feature of rote-learning, since such correction and modification suggests an activity closer to problem-solving than to rote-learning as it has been traditionally conceived. It is felt that the results of this experiment stress the essential continuity between the two activities which have often been regarded as distinct.

There was evidence that subjects found their continual repeating of the same mistake a frustrating process; they were trying to establish one order of responses and the material would not allow it. Often they became so convinced that the apparatus was at fault when their predicted response was not correct that they accused the experimenter of changing the key sequence. Such indications of emotional stress would suggest that a full explanation of error repetition would have to consider how a subject becomes "involved" in his own solution. This experiment can provide only descriptions of behaviour, but at that level it would definitely support the argument that a subject after learning a wrong sequence tries to "make" it the correct one.

Learning Involves Unlearning.

Learning seemed to be achieved finally when the subject had so firmly established the correct serial responses that the error could be located in its relationship to them; the schematic material had become "articulated" (Oldfield and Zangwill, 1938). Without such repetition the subject seemed unable to select from within the interrelated items and identify the locus of error. It was noticeable that during the course of learning a subject gradually identified an error which he had made persistently, and only after that did he learn the right response to replace it. In performing the task at this stage the subject literally moved to press the right key via the

wrong; his actions showed that the previously learned error was now serving as the cue for the more recently learned correct response. Thus one way of learning —and it was common enough although circuitous—was to link right and wrong together, so that it was *not* the isolated correct response which was learned, but the correct response related to an incorrect. The persistency with learned material of this correct to incorrect relationship, of the older to the more recent material, is again a fairly widespread phenomenon, and in a specialised field has been reported in prefrontal leucotomy cases.

For many subjects, therefore, learning was a process involving much unlearning and much amendment. As ever, the poorer performer was executing the harder task. He was spending much of his time trying to unlearn an error which he had himself brought into the task, and he still had to learn the correct response. A difference in the actual mental tasks which have been undertaken and achieved, seems to be some of the explanation for the pronounced individual differences which learning experiments always reveal.

I should like to thank Dr. A. D. Harris for much generous assistance in preparing this paper. Within the Nuffield Unit, the Director, Mr. A. T. Welford designed the apparatus and offered valuable suggestions in the course of the work.

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AN EXPERIMENT IN PROBLEM SOLVING

BY

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Trial-and-error problems are described in terms of "stimulus" difficulty, which is a measure of the number of possible modes of response left to the individual when all the information given is taken into account; and "phenomenal" difficulty, which is a measure derived from the individual's performance. An experiment is described in which three types of problem were presented to human subjects. In all three problems the stimulus difficulty was calculable, stage by stage, in the solution. The problems differed in this stimulus difficulty and also in the qualitative nature of the information provided—from unequivocal to conditional. It is shown that the qualitative difference of the nature of the information bears most relationship to phenomenal difficulty. Some observations are made on the modes of solution adopted, and further experimental work is suggested.

I

Introduction

The need for description of a task.

This experiment was performed as an attempt to describe in both quantitative and qualitative terms the behaviour of individuals faced with a "trial-and-error" problem solving task. It has long seemed to the writer that there is an inadequacy of description of tasks, and of stages in solution, in both problem-solving experiments and in learning experiments. Tasks are frequently described in terms of psychological theory or inferences from behaviour. As examples, the descriptive adjectives may be "trial-and-error" or else "insight." If, instead, we consider tasks as modified by knowledge of results in stages during the solution, we may find a more satisfactory mode of description, referring to the problems themselves, and not to theory nor even necessarily to behaviour. It may thereby become possible to establish an independent base line from which behaviour may be measured or described. Considering briefly the two types of problem mentioned above—"trial-and-error" and "insight"; it would appear that the "trial-and-error" problem is one in which there is a reasonably high probability of achieving some degree of success fairly early in the attempt at solution, and that both the correct and the incorrect elements of the partial success, if known and acted upon, constrain behaviour closer and closer to the completely correct solution. An attempt at solving an "insight" problem, on the other hand, is almost certain to be either completely right or completely wrong, and in the latter case, except for the narrow information that this specific attempt is wrong, very little information is gained. The problem remains approximately of the same difficulty until complete solution is obtained.

The term "difficulty" having been introduced, it would be as well to consider its meaning in this context. Its usual reference in psychology is derived from behaviour, measured by the mean time or mean number of trials required for solution or for fulfilling an accepted criterion—or alternatively, in terms of what proportion of a standard population under standard conditions obtain a solution. But it would seem possible, theoretically at any rate, to consider a more objective form of difficulty, associated purely with the problem and not dependent on the behaviour. To take an example, if we present a multiple-choice intelligence test question the difficulty of the question is dependent partially on the number of possible "answers" presented. This is merely stating that the task lies mainly in the choice; the judgment of the

fittingness of the question to each of the possible given answers. Other things being equal, the "difficulty" is a function of number of answers given. This is, of course, only partially true, in the sense that behavioural "difficulty" can be modified without changing the number of answers—i.e. if all except one of the given answers are obviously absurd then the task is simplified. But this is an entirely different notion of "difficulty." These two types of difficulty may perhaps be named "stimulus difficulty," referring to the possible descriptions or measurements inherent in the form of the task, and "phenomenal difficulty" referring to the perceptual and cognitive aspects of the content or the mental processes required for solution. The analogy with terms in the psychology of perception is deliberate. As in the study of perception, "stimulus" is intended to be described in objective or physical terms, and "phenomenal" in terms of the behaviour of the observer.

As mentioned earlier, the case of problem solving adds the further complication that difficulty is going to be affected by partial success and knowledge of results. At first sight it would seem not easy to allocate this change of difficulty to either stimulus or phenomenal difficulty, in that knowledge of results requires appreciation by the observer to be effective. However, if it is considered as information now made available, to be added to the basic information given in the problem, it can clearly be treated as a change of stimulus difficulty.

Thus for the study of problem solving it is desirable that the problems chosen should be such that a measure of stimulus difficulty can be made, and that changes in stimulus difficulty in the process of solution can also be measured. The phenomenal difficulty should be measurable from the behaviour of individuals. Further, the problems chosen ought to be such that qualitative observations, which may hint at the modes of solution attempted or the mental processes involved should be capable of record.

Design of suitable experimental problems.

Simple derivatives of matching tasks were devised as problems conforming to the suggested requirements. Let us consider the simple task in which four objects are to be matched against four spaces—or four other objects. For a first trial, there are 4! or 24 ways of arranging the objects. Thus the initial stimulus difficulty can be considered as a function of 24. Of these 24 arrangements, one is completely correct. In this case the stimulus difficulty ceases to exist, the problem is solved. Six of the possible first-trial arrangements give two correct matchings and two incorrect. We have now a great deal of information. For example, we may know that A goes on α, B goes on β, C does not go on δ, and D does not go on γ. Altogether this information adds up to the statement that the solution must lie in interchanging the two incorrect objects. Thus, instead of the initial 24 possible arrangements, there is now only one possible arrangement. The stimulus difficulty has been reduced to some function of one. The phenomenal difficulty need not of necessity be similarly reduced—the objects may so closely resemble each other that it is almost impossible to remember which object was where. Eight of the possible first-trial arrangements have one correct matching and three incorrect. One of these is

	1st trial		*2nd trial choice of*		
α	A		A		A A
β	C	leading to	B or D	→ either	B or D
γ	D	→	B or C		C B
δ	B		C or D		D C

—two possible arrangements, one correct, the other giving sufficient additional information for the correct arrangement to be the only possibility for the third trial.

And still further, nine of the possible first-trial arrangements are without any correct matches. But they still give information.

	1st trial		*2nd trial choice of*
α	B		A, C or D
β	C	leading to	A, B or D
γ	D	→	A, B or C
δ	A		B, C or D

which has nine possibilities. Tabulating the possibilities (continuing the correct solutions throughout) we have:—

(For the first trial the number of possible arrangements is 24.)

Trial 1		*Trial 2*		*Trial 3*		
Frequency	*No. of possible arrangements for Trial 2*	*Frequency*	*No. of possible arrangements for Trial 3*	*Frequency*	*No. of possible arrangements for Trial 4*	
1	0	12	0	23	0	All correct at Trial 4
6	1	10	1	1	1	
8	2	2	2			
9	9					

Thus we can see how the stimulus difficulty is changing, and the variation in change with the degree of success achieved.

The number of possible arrangements is not a very convenient measure to use directly. This will be obvious when one considers matching a greater number of objects, the distribution tends to be skewed very markedly, so much so that any simple average would not be meaningful and a transformed value is desirable. For the first trial, the number of possible arrangements is $n!$, where n is the number of objects to be ordered. This number is reduced trial by trial. It seems reasonable to reverse the factorial process; if x arrangements are now possible, for this to be measured as y, where $y! = x$. In the majority of cases x is not a true factorial, but the use of Γ function tables overcomes this. Thus "y" is the number, which if it could have a factorial, would have x as its factorial. This value y can be referred to as the "Equivalent Series," i.e. the problem is now at the same value of stimulus difficulty as would be presented for the first trial of a series of y objects. Thus after a first matching trial of four objects, none of which is correctly placed, $x = 9$, and the Equivalent Series is 3·31.

In one sense this is absurd, as 3·31 objects cannot be matched, but the transformation serves two useful functions. It restores the measure to a scale linearly related to the number of objects presented in the simple case (magnitude therefore can be simply appreciated). It also reduces skewedness so that simple statistical procedures are applicable.

The logarithmic transformation as used by Shannon is not as suitable as the Γ function in this case. With small probabilities of chance success, as in early trials with a large number of objects, the two measures are fairly closely related. But a logarithmic measure fails to differentiate between the condition of the problem being solved (E.S. = 0), and the condition of having all the necessary information to solve the problem (E.S. = 1). In terms of information this does not involve any confusion, but in terms of behaviour there may be, and often is, a difference between the two conditions. As the purpose of this experiment was to relate behaviour to information available, a single measure embracing both aspects of the problem was necessary.

First matching problem (*the* 8(1) *condition*).

The process of "calibrating" the stimulus difficulty for longer series of matching tasks is tedious. The measurement of the individual's performance merely involves calculating the number of arrangements possible taking into account the information gained, stage by stage. But an overall value is required for the task, and, strictly, this means taking each possible initial trial (or a representative of each class), counting the number of possible arrangements left, trying each of these (or again representatives) and seeing how many arrangements are left after the second trial and so on. Fortunately the task can be systematized. In the first place, the distribution of successes by chance on the initial trial is known. Secondly, if there are, say, four unsuccessful matchings in the first trial, then it does not matter how large the series is, the number of arrangements left is still the same: i.e. we may treat it as a series of four with no correct matches on the first trial. The computation thus is a matter of ascending from two incorrect matches on the first trial, to three, to four, etc. Later trials are more difficult, but can also be systematized.

This "calibration" has been performed for series of up to eight in length, eight being the value chosen for this experiment. With eight there are 8! possible arrangements (40320) on the first trial.

First trial frequency	*No. of arrangements for second trial*	*Equivalent Series*
1	0	0
28	1	1·00
112	2	2·00
630	9	3·31
2,464	44	4·39
7,420	265	5·45
14,832	1,854	6·50
14,833	14,833	7·52

Expected Mean E.S. after trial 1 = 6·487.

Following on with trial	2	Expected Mean E.S. is	4·656
"	"	"	"	3	"	"	"	"	2·508
"	"	"	"	4	"	"	"	"	0·810
"	"	"	"	5	"	"	"	"	0·128
"	"	"	"	6	"	"	"	"	0·006
"	"	"	"	7	"	"	"	"	0·000025
"	"	"	"	8	"	"	"	"	0·000

This provides the base line against which the average human performance can be measured. Any failures to use the information—either forgetting a correct match or repeating an incorrect match—meant that there were more arrangements left possible than there would have been had these errors not been introduced. This case of simple matching will be referred to as the 8 (1) condition of problem.

Second matching problem (*the* 4(2) *condition*).

A second matching problem using the same material was devised. Instead of matching eight objects to eight spaces, and being told after each trial which objects were correctly matched and which were not, eight objects were to be "placed," two in each of four cells. The information given, for each cell was whether there were 0, 1, or 2 objects correctly placed. If one was correctly placed and the other was not, the correct one was not specified, the information being that one was correct and one incorrect. The initial stimulus difficulty is less than in the 8(1) condition, as order within each cell is irrelevant. There are 2,520 initial possible arrangements, one sixteenth of 8!, the Equivalent Series being 6·65 (again referring to the equivalent as a simple matching problem). But the information gained from a trial may be conditional.

Consider the first trial:—

Correct	*1st Trial*	*Possible arrangements for 2nd trial*
AB	AE —1 correct	either A with B, C, D or F; or E with B, C, D or F.
CD	CF —1 correct	either C with A, B, D or E; or F with A, B, D or E.
EF	BD —0 correct	Two from A, C, E and F.
GH	GH —2 correct	G and H.

There are eight possible arrangements, but the A, E and the C, F alternatives may have to be remembered for subsequent trials. The first trial is straightforward, ranging from 1 arrangement completely correct, to 297 arrangements with no correct entry, and leaving 297 possible arrangements for the second trial. Problems of symmetry and of other forms of patterning make a systematic approach to the expected distribution somewhat laborious and the values given here may not be completely accurate.

Initial Equivalent Series	6·65
Expected Mean E.S. after trial 1	4·863
" " " " " 2	2·623
" " " " " 3	0·818
" " " " 4	0·079
" " " " " 5	0·0001
" " " " " 6	0·000

This problem is referred to as the 4(2) condition.

Third matching problem (the 2(4) condition).

The third variation of the problem is on *a priori* grounds simpler still. The eight objects are to be allocated, 4 to each of 2 spaces. There are only 70 ways in which this can be done. One of these is completely right, one is completely wrong, giving the information that a straight interchange of the objects is all that is necessary. As in the 4(2) condition, information is given as to the number correct in each cell—it is bound in this case to be the same for both cells. This is the 2(4) condition, and treating the problem in the same way as the others, we have:—

Initial Equivalent Series	4·68
Expected Mean E.S. after trial 1	3·908
" " " " " 2	3·095
" " " " " 3	2·156
" " " " " 4	1·200
" " " " " 5	0·355
" " " " " 6	0·110
" " " " " 7	0·016
" " " " " 8	0·000

In all conditions these values represent the calibration of the problems in terms of the average expected stimulus difficulty, trial by trial, throughout the performances. They represent the average of all possible performances by a machine which had perfect memory, but which otherwise acted purely at random. The performances of eighteen human subjects, measured in the same way, are compared with them.

Experimental material, instructions, and design.

The eight objects employed were:—

(1) Block of wood ($2\frac{3}{4}$ in. × $1\frac{3}{4}$ in. × $\frac{7}{8}$ in.) .. .. W
(2) An empty safety match box .. .. M
(3) A piece of brass plate ($2\frac{1}{2}$ in. × 1 in. × $\frac{1}{8}$ in.) .. .. P
(4) A piece of brass tube—like a napkin ring ($1\frac{3}{8}$ in. diam. × $\frac{13}{16}$ in. ht.) R
(5) A meccano 1 inch gear wheel .. .. G
(6) A wood screw ($2\frac{1}{2}$ in. long) .. .. S
(7) A bolt with hexagonal head ($\frac{3}{4}$ in. long × $\frac{1}{2}$ in. head) .. .. .. Bo
(8) A small metal bracket (1 in. × $\frac{1}{2}$ in. × $\frac{1}{2}$ in. wide) .. .. Bk

For the 8(1) condition a sheet of cardboard was divided into 8 spaces from left to right. For the 4(2) condition another sheet was marked out into quadrants, and for the 2(4) condition a sheet was divided into 2 cells, Left and Right. For an experimental session the subject sat at a table with one of the cardboard sheets on it, and the eight objects placed in a heap randomly beside it. In the 8(1) condition the instructions were, "Here are eight objects, and on the sheet of cardboard in front of you there are eight spaces. Each object has been allocated to a space at random, and your task is to find out which object goes where for them all. The procedure is this; after a trial (and a trial means all eight objects placed one in each space) I will place a card with the word "Correct" against each object which is in its correct cell. I will put nothing against an incorrectly placed object. You will then have half a minute to look at the display without manipulating it. At the end of that time I will take the cards away, shuffle the objects and return them to base and you will have another try. Except for the half minute for looking at the display there is no time limit."

Similar instructions were given in the 4(2) and the 2(4) conditions, except that they were told that 0, 1 or 2, or 0, 1, 2, 3 or 4 cards would be placed in appropriate cells, but that in the conditional cases they would be told not which objects were correct, but only the total correct in the cell.

Three random "correct" allocations were used and rotated between the conditions. The effects of order of trial (at least 48 hours interval between sessions was maintained) were minimized by a factorial design:—

i.e.	8(1)	4(2)	2(4)
Subject 1	1st	2nd	3rd
2	1st	3rd	2nd
3	2nd	1st	3rd
4	2nd	3rd	1st
5	3rd	1st	2nd
6	3rd	2nd	1st

Similarly for subjects 7–12 and 13–18.

For subjects 1–6 the correct orders were:—

8(1)	Bk	R	M	P	G	Bo	W	S	
4(2)			G	W	P	Bk			
			S	R	M	Bo			
2(4)	M	P	G	S	W	R	Bo	Bk	

for subjects 7–12:—

8(1)	G	W	P	Bk	S	R	M	Bo	
4(2)			M	P	G	S			
			W	R	Bo	Bk			
2(4)	Bk	R	M	P	G	Bo	W	S	

and for subjects 13–18:—

8(1)	M	P	G	S	W	R	Bo	Bk	
4(2)			Bk	R	M	P			
			G	Bo	W	S			
2(4)	G	W	P	Bk	S	R	M	Bo	

The eighteen subjects were students, research workers and staff from the Psychological Laboratories at Cambridge and University College, London.

Thus the experiment is balanced for straightforward comparison between the three conditions. Record was made of where each object was placed in each trial. The number of possible arrangements left at the end of each trial was computed and transformed into the Equivalent Series.

II

Results

The mean equivalent series, trial by trial, is shown for the three conditions in Table I. It will be seen that, compared with expectation, human performance in

TABLE I

Performance of Subjects compared with Theoretical Performance

Equivalent series after trials

Trials	*Theoretical*	*Observed*
8(1) Condition:		
1	6·487	6·491
2	4·656	4·701
3	2·508	2·726
4	0·810	1·183
5	0·128	0·462
6	0·006	0·111
7	0·000025	0·056
8	0·000	0·000
4(2) Condition:		
1	4·863	4·785
2	2·623	3·244
3	0·818	1·525
4	0·079	0·667
5	0·0001	0·333
6	0·000	0·222
7		0·167
8		0·056
9		0·056
10		0·056
11		0·056
12		0·000
2(4) Condition:		
1	3·908	3·993
2	3·095	3·143
3	2·156	2·421
4	1·200	1·401
5	0·355	1·179
6	0·110	0·778
7	0·016	0·722
8	0·000	0·722
9		0·667
10		0·556
11		0·389
12		0·333
13		0·278
14		0·278
15		0·222
16		0·111

the 8(1) condition is reasonably good. But it is considerably poorer in the 4(2) and the 2(4) condition. Phenomenal difficulty in the latter two conditions would seem to outweigh stimulus difficulty to some extent. Nine subjects in the 8(1) condition behaved with perfect consistency—i.e. they never repeated an error nor forgot a correct match. Three subjects did so in the 4(2) condition and only 2 in the 2(4)

condition. Similarly, as a crude indication of failure to take account of all information, we may consider those instances where complete information had been obtained (i.e. if full account was taken of all information, the next trial would be completely correct) but where the subject failed to respond and produced a wrong arrangement.

In the	8(1)	condition	6	S's	made	1	unnecessary	trial
"	"	"	"	1 S	"	2	"	trials
"	"	4(2)	"	4 S's	"	1	"	trial
"	"	"	"	2 "	"	3	"	trials
"	"	"	"	1 S	"	4	"	"
"	"	"	"	1 "	"	7	"	"
"	"	2(4)	"	3 S's	"	1	"	trial
"	"	"	"	1 S	"	5	"	trials
"	"	"	"	4 S's	"	6	"	"
"	"	"	"	2 "	"	7	"	"
"	"	"	"	2 "	"	11	"	"

In the 2(4) condition two of the subjects failed to find a solution in 16 trials, at which point the experimental sessions were suspended.

The observations can be alternatively described in terms of the number of trials required for solution, compared in all cases with the number of trials expected on a hypothesis of perfect memory, but otherwise random behaviour.

TABLE II

Observed and Expected Number of Trials for Solution

	8(1) *Condition*		4(2) *Condition*		2(4) *Condition*	
	E.	O.	E.	O.	E.	O.
1st	·000	0	·007	0	·257	0
2nd	·110	0	·429	0	1·029	0
3rd	1·916	1	5·700	1	2·057	1
4th	6·975	5	10·454	6	4·038	4
5th	6·975	6	1·408	5	5·611	2
6th	1·916	4	·002	2	3·572	2
7th	·110	1	—	1	1·131	
8th	·000	1	—	2	0·285	
8+	—		—	1	—	9

An additional behavioural measure can be made in terms of survival of information. If we take the first trial, this has given some information, and any subsequent trial can be tested against it to see whether it is consistent, or infringes the information gained in trial 1. If the later trial is consistent, we have no real way of deciding whether this was deliberate or not, whether the subject had remembered and was using the information gained earlier, or whether the consistency had occurred by chance or was derived from information gained in intervening trials. If, however, the trial is not consistent, then we can assume either that the subject has forgotten the information, or for some reason is deliberately refraining from using it. Thus we can take, trial by trial, a measure of the survival of information. On the parallel with other survival distributions, we can assume this to be quasi-logarithmic in form, and take either the logarithmic decrement, or the half period as the measure of survival.

TABLE III

SURVIVAL TIME OF INFORMATION

8(1) *Condition*		4(2) *Condition*		2(4) *Condition*	
Trial	½ *period (in trials)*	*Trial*	½ *period*	*Trial*	½ *period*
1	6·81	1	2·41	1	1·71
2	4·96	2	1·65	2	1·33
2+	5·02	3	1·62	3	1·00
		3+	2·95	4–8	1·37
				8+	2·23
All	5·55		2·16		1·55

This agrees with the other quantitative data in indicating the relative phenomenal difficulty of the tasks. The differences between the trials may just be a normal memory effect, as shown in Ebbinghaus' work on serial learning. It may alternatively be a function of approach to the problem by some individuals who deliberately referred back to their first trial after subsequent trials, testing the observations gained against it and paying less attention to intervening trials. The improvement in consistency with later trials may be due to the fact that the problems were nearer solution, and most of the information was now systematized.

Behaviour in the first trial.

The first trial differs from the succeeding trials in that the subject has no information to go on at all. He has been told that the correct matchings have been assigned by chance, and might therefore be expected to respond in an equally random manner. He does not in general do so.

In the 8(1) condition, the arrangement

1	2	3	4	5	6	7	8	
W	M	P	.	.	.	.	.	was given by 7 subjects
W	P	M	.	.	.	.	.	" " " 2 "
.	.	.	.	.	P	M	W	" " " 3 "

Thus 12 out of 18 subjects adopt some rudimentary form of a size or size and shape classification for their first trial.

Similarly in the 4(2) condition, in the first trial:—

W and M in same cell	12 subjects
R and G in same cell	9 "
P and Bk in same cell	8 "
S and Bo in same cell	8 "

5 subjects had all 4 pairs
6 " " 2 of the pairs
4 " " 1 pair
and 3 " " 0 pairs.

And further, in the 2(4) condition, 6 subjects divided the objects into W M P Bk on one side and R S G Bo on the other (a shape classification—square v. round). A further 5 had W M P on one side with some object other than Bk.

It will be seen how important it was to interchange the random "correct" layouts between the three conditions, as there obviously exist commonly preferred orders, and one of the random arrangements might be nearer to these than the others.

III

Discussion and Qualitative Observations

The first conclusion to be drawn from the observations is that phenomenal difficulty (measured from the behaviour) and stimulus difficulty (measured in the manner described) are not in agreement. It is suggested that this difference lies primarily in the nature of the information gained by the subject, varying as it does from the unequivocal "correct-incorrect" information in the 8(1) condition, through the simple conditional form—the either this is right or that is right—sometimes found in the 4(2) condition, to the more complex conditional form of one or two or three out of four are correct, found in the 2(4) case. Furthermore it is suggested that these differences force the subjects to adopt different methods of solution for the three tasks. A feature common to all three tasks is the attempt on the part of the subjects to impose methods of or aids to solution on to the task. The first of these has already been mentioned; the laying down of the material on the first trial in some sort of meaningful order. The advantages of this procedure are obvious: instead of having to remember the position of eight objects, all that is necessary is to remember the basis of the classification. It is the introduction of a generalization, not as a method of solution, but as a means of organizing, and consequently of simplifying the data. The majority of subjects adopted some form of classification, easily identifiable from the records, in all three tasks. In the 8(1) condition, many subjects adopted additionally a systematic approach to the solution, the best such system being "scanning." This is best illustrated by example.

Correct order	M	P	G	S	W	R	Bo	Bk	
1st trial ..	*W*	*M*	*P*	*R*	*G*	*S*	*Bk*	*Bo*	(size classification)
2nd trial ..	*Bo*	*W*	*M*	*P*	*R*	*G*	*S*	Bk	(moved one to right)
3rd trial ..	*S*	*Bo*	*W*	*M*	*P*	R	*G*	Bk	(continued)
4th trial ..	*G*	*S*	*Bo*	*W*	*M*	R	*P*	Bk	»
5th trial ..	*P*	*G*	*S*	*Bo*	W	R	*M*	Bk	»
6th trial ..	M	P	G	S	W	R	Bo	Bk	»

Nine subjects used this method from the beginning, two others took it up after early trials. Some other systems adopted for part or all of the time were reversal of trial 1 order or interchange of adjacent pairs. In no case did there seem to be a complete absence of system of some form or other.

The combination of an initial classificatory layout, and systematic change between trials reduces the phenomenal complexity of the problem enormously, the only problem remaining is memory of the items correct—the others are taken care of. It is perhaps worth noting that two individuals, faced with a sudden large increase in the number correct on one trial, broke down on the system. One subject failed to remember an item which had been in its correct position for two trials, and the other failed to move one of the incorrect objects to its next scanning position. These may represent the effect of change from a set to avoid repeating errors to a set to retain the correct responses, but more evidence would have to be forthcoming for any definite conclusion.

The two other tasks, however, do not lend themselves to such systematic solution. In the 4(2) condition, several individuals attempted a systematic approach, even though it ignored some of the information.

As an example:—

	M	P	G	S	W	R	Bo	Bk
Correct ..	M	P	G	S	W	R	Bo	Bk
Trial 1 ..	*Bo*	*Bk*	*P*	S	*G*	*M*	*R*	*W*
" 2 ..	*Bo*	*W*	*Bk*	S	*G*	*P*	*R*	*M*
" 3 ..	*Bo*	M	*W*	S	*G*	*Bk*	*R*	*P*
" 4 ..	P	M	*Bo*	S	*G*	W	*R*	Bk
" 5 ..	P	M	*R*	S	*Bo*	W	*G*	Bk
" 6 ..	P	M	G	S	R	W	Bo	Bk

In this case the information from the first trial (i.e. that except for either P or S, none of the objects was in its correct position) was not wholly employed. Instead, Bo, S, G and R were left in position and the others were scanned.

This may have an advantage, in cases where several cells have no correct entry, in making any information gained unconditional—as appears in the example given, at trial 3, where M is certainly placed. But if there are cells initially containing one correct object, certainty is not achieved. In the example, trial 2 gives one correct entry in cell 2, as had been the case with trial 1. Although on a probability basis it is likely that this is S, it is not certain, as the same information would have been given were P and Bk the correct objects for that cell. A system like this, or some of the other less definite forms observable in the records implies a surrender of information, reducing the chance of early success in an endeavour to reduce the memory load of the task. Records of eight subjects indicate similar non-acceptance of information in keeping with some form of system.

Loss of information is even more marked in the third 2(4) condition, though it is less easy to relate this to any systematic approach. Rather than producing a system, and acting on it, individuals seem to make a hypothesis as to which objects are correct, and to test this hypothesis, and modify it trial by trial. This involves reference back to earlier trials. In a few cases trials appear to be referred to the first trial only. An example

Correct order ..	G	W	P	Bk	S	R	M	Bo	*Trial consistent with trials*
Trial 1 ..	*M*	W	P	Bk	S	R	*G*	Bo	
" 2 ..	*M*	W	*R*	Bk	S	*P*	*G*	Bo	1
" 3 ..	*M*	W	P	*Bo*	S	R	*G*	*Bk*	1, 2
" 4 ..	*M*	*S*	P	Bk	*W*	R	*G*	Bo	1, 2, 3
" 5 ..	*M*	*Bo*	P	Bk	S	R	*G*	*W*	1, 2
" 6 ..	*M*	*Bo*	P	Bk	S	R	*G*	*W*	1, 2 (repetition trial 5)
" 7 ..	G	*M*	P	Bk	S	R	*W*	Bo	1, 2, 3
" 8 ..	*Bo*	*M*	P	Bk	S	R	*W*	*G*	1, 2, 7 (rep. of trials 5 and 6)
" 9 ..	*R*	*M*	P	Bk	S	*W*	*G*	Bo	1, 3, 7
" 10 ..	*R*	*M*	P	Bk	S	*W*	*G*	Bo	1, 3, 7 (rep. trial 9)
" 11 ..	*M*	W	P	*S*	*Bk*	R	*G*	Bo	1, 2, 4, 5, 6, 8, 9, 10
" 12 ..	*M*	*S*	P	Bk	*W*	R	*G*	Bo	1, 2, 3, 7 (rep. of trial 4)
" 13 ..	*Bo*	W	P	Bk	S	R	M	*G*	1, 2, 4, 9, 10, 11, 12
" 14 ..	*S*	W	P	Bk	*G*	R	M	Bo	1, 2, 3, 5, 6, 8, 9, 10, 13
" 15 ..	*R*	W	P	Bk	S	*G*	M	Bo	1, 3, 4, 5, 6, 8, 11, 12, 13, 14
" 16 ..	G	W	P	Bk	S	R	M	Bo	Consistent with all

Every trial is consistent with the first trial. The second trial also has some effect. But every move was just another attempt at solving the problem as left at the end of trial 1. This can be seen from the repetitions of trials—particularly the two instances of immediate repetition. It is worth noting that, had the information been accepted, the problem would have been solved on the fifth trial. It is also worth noting that there are only 16 possible arrangements consistent with trial 1, and it is somewhat surprising, given this consistency with the first trial, that the solution was not reached earlier as a matter of chance.

Another method of attempted solution lay in referring trials back to their immediate predecessors, neglecting information obtained earlier. As an example:—

										Trial consistent with trials
Correct order	..	M	P	G	S	W	R	Bo	Bk	
Trial 1	..	M	P	*W*	*Bk*	*S*	R	Bo	*G*	
" 2	..	M	*W*	*Bo*	S	*P*	R	*G*	Bk	1
" 3	..	*W*	*Bo*	G	*Bk*	*M*	R	*P*	*S*	1, 2
" 4	..	M	*Bo*	G	*R*	W	*P*	*S*	Bk	2
" 5	..	M	*Bo*	G	*Bk*	W	R	*P*	*S*	1, 2
" 6	..	M	*W*	*R*	*Bk*	*P*	*S*	Bo	*G*	2, 4, 5
" 7	..	*R*	*Bo*	G	S	W	*M*	*P*	Bk	2, 5, 6
" 8	..	*W*	P	*R*	*Bo*	*M*	*S*	*G*	Bk	1, 2, 4, 7
" 9	..	M	*Bo*	G	S	W	R	*P*	Bk	6, 8
" 10	..	M	*Bo*	G	*R*	W	*P*	*S*	Bk	2, 9 (repetition trial 4)
" 11	..	*Bo*	P	G	S	W	R	*M*	Bk	2, 4, 5, 9, 10
" 12	..	M	P	*Bo*	S	W	R	*G*	Bk	1, 3, 4, 5, 6, 9, 10, 11
" 13	..	M	P	G	S	W	R	Bo	Bk	all

In this case, except for trials 4 and 5, all the trials are consistent with the trial immediately preceding, and several are consistent with the two immediately preceding trials, but on the whole information gained earlier than that is ignored. This procedure stands in contra-distinction to the example given immediately above, and yet there is one important feature in common. These individuals (like almost all of the subjects) either could not or would not act upon all the information. The difference lies in their selection of information.

In a few instances there were examples of assumptions made which were erroneous. Some of these cases bear striking resemblance to fixated behaviour more usually mentioned in connection with experimental studies of conflict. As an example:—

Correct order	..	Bk	R	M	P	G	Bo	W	S
Trial 1	..	*G*	R	*W*	P	*Bk*	Bo	*M*	S
" 2	..	*Bo*	*S*	*W*	P	G	*R*	*Bk*	*M*
" 3	..	*G*	R	*S*	P	*Bk*	Bo	W	*M*

At this stage the subject decided that G and R should be placed in the left hand cell. For the next eleven trials these two objects were taken up first and placed confidently on the left, the subject then studying the remaining six objects. After the fourteenth trial this broke down, and G was correctly transferred to the right hand cell. Repeatedly during the eleven trials information was presented which indicated that both G and R could not be on the left, but it was ignored.

If an analogy with another type of problem is possible, these three examples are like three different attempts at a jigsaw puzzle. The first is like taking one piece and trying the other pieces against it; the second like taking one piece and trying another against it, taking a third piece and trying it against the second, and so on; whereas the third example is rather like deciding that "this brown piece must be part of the horse's head."

Another interesting feature of the observations on this condition is that behaviour, with respect to the information of correct or incorrect does not correspond to the actual value of the information given. As the objects are to be placed in either of two cells, it follows that the information that an object is incorrectly placed is equal in value to the information that it is correctly placed—merely requiring the subject to reverse its position. Information that none of the objects are in their appropriate cells is complete information. Three subjects reached this position. Two failed to act upon the information, although a subsequent report from one subject implied that this was a memory failure rather than a failure to appreciate the value of the information. The third subject responded correctly, after a long pause, and with the

observation "Would I be naïve if I thought that if I reversed them they would all be right?"

Similarly, information that one object in each cell was correctly placed is equal in value to information that three objects are correctly placed in each cell. Both leave only sixteen possible arrangements, whereas the condition of two right in each cell leaves 36 arrangements. Behaviour was not consistent with this, however, for if we take survival times of information from trials with these "success" scores we have:

1 correct each cell									½ period of survival of information	0·72 trials
2	"	"	"	"	"	"	"	"	"	1·09 "
3	"	"	"	"	"	"	"	"	"	4·84 "

The phenomenal difficulty of the task after a one correct trial is thus much greater than after a three correct trial even though the stimulus difficulty is the same. This may mean that the value of "correct" is increased over "incorrect" by the emotional implications of the terms. Or it could merely be a carry over from more normal problem situations (as in the two other experimental conditions) where this equality of value in terms of stimulus difficulty does not hold. Or as a third possibility it could be that the subjects' set was towards the placing of four correct objects into a cell, rather than the placing of each object into one of the two cells, and that the complementary nature of the two cells, being one stage removed in the formulation of the problem, did not affect their behaviour.

The nature of trial and error problem solving.

The nature of the trial-and-error problem has already been discussed, with its emphasis on the progressive reduction of number of possible modes of behaviour during the course of solution. Ideally, the aim of the solver—human, animal or mechanical—is to make this reduction as efficiently as possible. The experimental results indicate that the human subject, under some conditions, does not show mechanical efficiency. Some speculation is possible as to the nature of this distinction and its relation to efficiency in the psychological sense of use of the subject's own perceptual, memory and cognitive abilities. The number of objects used in these experiments was partly chosen because of its divisibility into the three experimental conditions, and also partly as it is about the normal maximum for immediate memory span for position. Twelve objects would have suited the experimental design, even more so than eight, but even trial-to-trial consistency would have been problematic, in the face of the immediate memory task involved. Even with eight objects it is doubtful whether the normal individual's ability to remember would, by itself, have enabled him to solve any of the problems. Thus memory aids, or a judgment of the important information to be remembered are a necessity. The most customary memory aid, notes on paper, was denied. The initial classification of objects, observed in all experimental conditions, and the systems such as scanning, in the first condition, are very good substitutes. The simple matching problem is so greatly reduced in phenomenal difficulty by them that it almost ceases to be a problem, and is certainly the easiest of the three. The memory load is reduced to immediate memory of correct and incorrect and direction of scanning for each trial only, without any necessity to carry information over longer periods by sheer memory. The two generalizations, of order and of movement together take up all the information and reduce it in its psychologically quantitative size to an amount easily coped with by the individual. The same essential feature is observed in the other conditions, only in these cases the reduction has to be made by the selection of information —after the initial memory aid of classification of objects. This selection takes place in different ways, mainly by selecting some of the objects and ignoring the others,

or by selecting some of the trials and ignoring others, or by selecting some inferences from trials and ignoring others. In real life problem solving situations, in addition to these methods of selection (which may be persistent behavioural characteristics of the individuals) there are usually preformed evaluations of the value of different pieces of information, evaluations carried over from previous experience or training, etc. This only differs in the experimental situation in that the experiment has an initial trial, which can be considered as effectively absent in the real life situation.

It could be paralleled experimentally, by the use of material in which there were inherent clues as to the correct position; by instructing the subjects that there was some sense in the arrangement not just a random allocation. Some earlier experiments showed that inherent clues produced a greater chance of early success, but if this were not attained they interfered with any later solution. These experiments were performed using the simple matching problem only (like the 8(1) condition); it would be of interest to see whether, in the more conditional cases the inherent clues took greater priority of attention.

It is tempting to speculate on the relation of intelligence to these describable aspects of problem solving. For a functional definition, intelligence is bound to be concerned with ability to solve problems, but the working definitions on which most measurements of intelligence are based restrict it either to an appreciation of the formal structure of problems, or the ability to comprehend and manipulate all the information for a series of simple problems. The choice of generalization, or the process of selection and rejection of information are very rarely demonstrated in intelligence tests, and yet it may be that these are far more important in the normal use of intelligence; the production of a tentative "solution" to problems, the formal structure of which escapes comprehension.

The experiment described does not throw any light on this. Its design, balanced with respect to the three conditions, makes it difficult to compare subjects. In addition, the variation in chance success in the first trial, assumed to be balanced as far as conditions are concerned by using eighteen subjects and rotating the experimentally "correct" orders between the conditions, is bound to lower any correlation of subject's performance between the three conditions. An experiment specifically aimed at this end is perfectly feasible. It would presumably require more subjects, and some standard system of constructing the "correct" order from the subject's first trial devised. It ought then to be seen whether there was any correlation in performance, and perhaps more important, any persistence in the qualitative methods of solution adopted. The observations would have to be validated against an external criterion of degree of success in coping with real life problems, if anything of the nature of intelligence is to be associated with the performance.

The writer wishes to thank his colleagues in the Psychological Laboratories at Cambridge University and University College, London, for their willingness to act as subjects, and their helpful discussion of the observations.

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BOOK REVIEW

Handbook of Experimental Psychology. Ed. S. S. Stevens. London: Chapman and Hall, 1951. Pp. xi + 1,436. 618 illus. £6.

Julian Huxley, in his "Essays of a Biologist," may have first set down the view that "The biologist cannot fail to be impressed by the fact that his science to-day is . . . in the position which Chemistry and Physics occupied a century ago. It is beginning to reach down from observation to experimental analysis, and from experimental analysis to grasp of principle." If there is no *a priori* case for the same optimistic view of psychology, the recent success of a few less ambitious books at least makes the position arguable, and the logic for a compendious survey of the facts and an examination of the theories proposed to account for them should not be too difficult to find. Such a handbook must, however, justify itself not only in its occasion but also by its scope, having more to contribute than a collection of reviews from the Psychological Bulletin and yet lacking the partisan selection of material which goes with a narrower intention. It was the achievement of Murchison's 1934 handbook, now long out of print, to provide the research worker with a mine of information and at the same time, by virtue of the balance struck between the persuasions of its various contributors, to give a much used, if seldom quoted, baseline for the research of the next decade. We should not, then, be surprised if its successor fell short of this high standard; that the present volume falls so far below is disappointing. With some notable exceptions where the writer has been aided by the quality of his subject, the limitations of space conspire with the intention to include all normal experimental psychology, and much else besides, to give the reader an impression not so much of superficiality as of an uneasy fluctuation on the part of the contributors between the lexicographic completeness of a Catalogue of Ships and the condensed obscurity of the Funeral Oration. The effect is most marked in the section devoted to learning, which should form the core of such a book as this, and while it is a matter of bias whether one would sacrifice some of the physiological detail or the four chapters given over to applied psychology, it is clear that a more radical operation than Stevens ever contemplated is needed if the experimental psychologist is to come to terms with the economics of publishing.

The literature on the special senses lends itself more readily than any other to a review of the kind attempted in this book, and so it is that the papers of Judd, Bartley and Graham on the physics, physiology and psychology of vision, and an impressive parallel contribution mainly from the Psycho-Acoustic Laboratory at Harvard contrive to be both concise and informative. Of particular interest is the review by von Békésy and Rosenblith of the senior author's work on the "Mechanical Properties of the Ear" and a chapter by Licklider and G. A. Miller on the "Perception of Speech." Bartley's summary of research since 1934 serves to correct the more speculative account given by Hecht in the previous handbook, though it is curious that neither of his sections on visual acuity gives any reference to Marshall and Talbot's contribution to this problem. Graham has an admirably clear account of the literature on the perception of space and movement, together with a factual review of the Köhler after-effect.

This seems to be the best place to comment on Stevens' introductory paper, "Mathematics, Measurement and Psychophysics." As a statement of his own position it is less adequate than his previous writings, and his dismissal of Campbell's arguments perhaps a little cavalier. A critical analysis of topological concepts, to which he gives passing reference, and some account of the sociometric techniques of Guttman and Coombs would have added to the value of this chapter.

While the post-war years have witnessed an increasing scrutiny of some of the fundamental ideas of learning theory, the temper of the previous decade was, with the few exceptions when *expectancy* and *response* theories made contact, to accumulate facts and neglect their interpretation. The renewed interest in secondary reinforcement, the goal gradients, operant conditioning and so on is of too recent an origin to have influenced the character of the reviews in this book, and so it is something of a mixed blessing that the weight of evidence at present favours an S-R rather than any other theory: we are spared the wordy discourse of the Gestalt school only to find a too ready indulgence in the anomalies of Hull's position. We miss the Lashleian eye.

There are two chapters in the section on learning which, if we accept the limitations imposed upon them by space, are well worth reading. Spence gives a very fair account of Hull, Tolman, Skinner and Guthrie, and of the issues between them. Brogdens's review of the conditions affecting the acquisition and transfer of CR is excellent, though

this has been achieved at a considerable price, discrimination- and maze-learning receiving only the most summary treatment. Human learning is an untidy subject, and Hovland, by devoting a page or two to each topic *seriatim*, does not warm to his subject until he gets to reminiscence and transfer. The English reader will be disappointed to find little or nothing on remembering, recognition and the effects of set. Of the other chapters in this section, Hilgard on "Method" and C. T. Morgan on "The Psychophysiology of Learning" are adequate, while the papers of Leeper on "Cognitive Processes" and G. A. Miller on "Speech and Language" are less impressive. It will be sufficient if we comment on a chapter of similar quality by N. E. Miller on "Learnable Drives and Rewards" which follows upon Beach's interesting review of studies of reproductive behaviour in a section on motivation. The importance of secondary reinforcement to Hull's theory is such that the inclusion of a chapter devoted to the subject needs no justification. (Indeed the conception of organizing a series of papers on learning by subject rather than by species is one that might have commended itself to the Editor.) Miller's contribution, however, strikes us as incoherent in arrangement and not always convincing in its argument.

We are in no way qualified to comment in detail on the remaining two sections of this handbook. Brink and Ruch contribute two chapters each on neuronic and cerebral mechanisms respectively, and Dempsey completes the physiological section with a paper on "Homeostasis." The other, entitled "Growth and Development," contains an interesting review by Sperry of research on neurogenesis. C. S. Hall's survey of genetics provides the uninitiated with some useful comments on method in this field together with a short bibliography of some of the literature. Nissen's paper on interspecific comparison contains many references, omitted by the other contributors, which are relevant to motivation and learning, but his argument sometimes gets lost in the midst of these digressions.

In conclusion we must note the inadequacy of the subject index to this handbook. For instance, a long and useful footnote by Hovland on the learning of meaningful material (p. 622), and his paragraph on changed set (p. 677), are not referred to, and the tendency of different authors to use different terms, such as negative adaptation and habituation (pp. 364–366), is not always allowed for by cross references.

The present *Handbook of Experimental Psychology* is, in the jargon of the book reviewers, likely to remain the standard work for a number of years. While this assumption underlies the tenor of the present account, our criticisms should not be allowed to conceal the fact that it will be of considerable use to the research worker and, perhaps, to the less dyspeptic among the undergraduates. Nevertheless, all those familiar with "Murchison" will come with mixed feelings upon the enigmatic entry in the Subject Index—"It, antecedent of, removal by Editor, 293." P. H. R. J.

ERRATUM.—In: Szafran, J. Changes with age and with exclusion of vision in performance at an aiming task, the concluding paragraphs on page 112 from "When the distributions of all time measurements . . ." to ". . . developed by Kendale (1948) and Whitfield (1947)." on page 113 should be read as a legend to Table I on page 113. The paragraph immediately above Table II on page 114 beginning with "The differences in the values . . ." should be read as a legend to Table II.

INDEX OF AUTHORS

Page

CANE, V. R., with Horn, V.
The timing of responses to spatial perception questions - - - - 133
CARPENTER, A.
A case of absolute pitch - - - - - - - - - - - 92
CARTIER, F. A., with Denes, P.
Difference limina for quality of pure tones: an exploratory study - - 89
DANZIGER, K.
The operation of an acquired drive in satiated rats - - - - - 119
DEES, V., with Grindley, G. C.
The effect of knowledge of results on learning and performance: IV. The direction of the error in very simple skills - - - - - - 36
DENES, P., with Cartier, F. A.
Difference limina for quality of pure tones: an exploratory study - - 89
DEUTSCH, J. A.
A preliminary report on a new auditory after-effect - - - - - 43
DREW, G. C.
Variations in reflex blink-rate during visual-motor tasks - - - - 73
GIBBS, C. B.
Transfer of training and skill assumption in tracking tasks - - - 99
GRINDLEY, G. C., with Dees, V.
The effect of knowledge of results on learning and performance: IV. The direction of the error in very simple skills - - - - - - 36
HARRIS, N.
A note on the paired comparisons method of rating - - - - - 146
HICK, W. E.
A simple stimulus generator - - - - - - - - - - 94
HINSHAW, J. R., with Sinclair, D. C.
Sensory phenomena in experimental nerve block - - - - - - 49
HORN, V., with Cane, V. R.
The timing of responses to spatial perception questions - - - - 133
KAY, H.
Learning of a serial task by different age groups - - - - - - 166
LANGDON, J.
The perception of a changing shape - - - - - - - - - 157
MITCHELL, M. J. H., with Vince, M. A.
The direction of movement of machine controls - - - - - - 24
PHEMISTER, M. R.
An experimental contribution to the problem of apparent reality - - 1
PRETTY, R. G. F., with Russell, R. W.
A study of position habits induced by reward and "frustration" - - 151
RUSSELL, R. W., with Pretty, R. G. F.
A study of position habits induced by reward and "frustration" - - 151
SINCLAIR, D. C., with Hinshaw, J. R.
Sensory phenomena in experimental nerve block - - - - - - 49
SZAFRAN, J.
Changes with age and with exclusion of vision in performance at an aiming task - - - - - - - - - - - - - 111
Erratum - - - - - - - - - - - - - - 199
VERNON, M. D.
Learning and understanding - - - - - - - - - - 19
VINCE, M. A., with Mitchell, M. J. H.
The direction of movement of machine controls - - - - - - 24
WHITFIELD, J. W.
An experiment in problem solving - - - - - - - - 184

OCCUPATIONAL PSYCHOLOGY

CONTENTS of October, 1951, issue

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by Edith O. Mercer

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by The Foreman Research Group, N.I.I.P.

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